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MOTOR AUTOMATISMS AND REFLEX ACTION

BY FRANKLIN FEARING
Stanford University

A considerable degree of confusion seems to exist with regard to terminology in the field covered by the title of this paper—a confusion which doubtless reflects the difficulties in the way of interpretation of the phenomena themselves. The terms "reflex," "involuntary," "automatic," "automatism," "spontaneous," have been used interchangeably in referring to a type of action which may be characterized very broadly by the absence of or a low degree of "central" control. A review of physiological and psychological literature reveals uncertainty even with regard to a concept so well established as that of reflex action. Certain characteristics of the reflex, however, seem to be generally accepted, as for example, its brief *time*, its relative *independence of consciousness and volition*, its *predictability, i.e.*, the certainty of appearance when a particular stimulus is presented to the receptor, and its *purposiveness* in the sense that it serves some protective or adaptive end. These characteristics may be applied also to the automatism. Actions of this character are "automatic" in the sense that they are relatively free from central control.

The term "automatic," on the other hand, has been applied frequently to responses, the stimulation of which are *internal* rather than peripheral; this is the usage of Bayliss (**8**, p. 547), Tigerstedt (**83**, p. 570), and Foster (**25**, pp. 1078, 1216). Tigerstedt includes the "products of decomposition and internal secretion" present in the blood and lymph as sources of automatic excitation to the nerve

cells. Foster calls attention to the "spontaneous" movements of pigeons in the absence of the cerebral hemispheres, and suggests that the term "mechanical spontaneity" may be applied to these movements.

"And we may here perhaps remark that all these facts seem to point to the conclusion that what may be called mechanical spontaneity, sometimes spoken of as 'automatism,' differs from the spontaneity of the 'will' in degree rather than kind. Looking at the matter from a purely physiological point of view (the only one which has a right to be employed in these pages), the real difference between an automatic act and a voluntary act is that the chain of physiological events between the act and its physiological cause is in the one case short and simple, in the other long and complex." (p. 1078)

Bayliss discusses the question of whether or not the constant activity exhibited by the respiratory and vasomotor centers is "actually automatic," or whether it is the result of afferent impulses from the periphery. Such impulses are able to modify the state of these centers, but he states that it "seems possible" that a state of automatic activity may exist, although it is difficult to differentiate it from excitation which is the result of "stimulating substances in the blood."

As distinct from these usages, the term "automatism" is applied in psychology to those motor phenomena which are not conditioned by consciousness. Tucke in his "Dictionary" (86) defines automatism as follows:

"A state in which a series of actions are performed without cerebral action or conscious will, as during reverie or in certain morbid conditions."

Baldwin's Dictionary (4) gives the following definition:

"The performance of actions apparently involving some degree of psychological determination, without consciousness of the personal subject."

The same authority defines automatic action "in psychology":

"A succession of acts in response to repeated or continuous excitation, proceeding in more or less complete independence of attention."

The adjective automatic, however, is applied

"To those functions of the living organism which are independent of external stimuli, finding their stimulus in the conditions of the organism itself."

Richet in the "Dictionaire" (73) gives a somewhat more re-

stricted definition. He divides movements into four classes: (1) reflex: determined by exterior stimuli; (2) automatisms: determined by interior stimuli which are voluntary; (3) machine-like (machinaux): determined by the will, but continue without the intervention of the will; (4) volitions: determined and continued by the will. The interior stimulus necessary for the automatism is not the will, although it is psychical in nature; it is a "volition accompanied by complete amnesia." He points out that such movements are rare in the normal individual, but are found in somnambulistic states, delirious states, intoxication, mediumistic phenomena (table tipping, automatic writing, etc.), and hypnotism. Richet sees some resemblance between the movements of the automatic writer and those of the pianist who is able to converse while playing the piano. This application of the term "automatism" to certain phases of motor habits indicates an extension of the concept to cover a type of involuntary action which is not abnormal and which is not reflex in the usually accepted physiological sense.

A brief survey of the standard physiological texts indicates that the physiologists are not inclined to sharply differentiate the two kinds of involuntary action, namely, reflexes and automatized habits.

Howell (**33**) sees in the reflex an expression of the automatic activity of a mechanism. "Repeated experiences" of the individual as well as inheritance may account for the existence of neural paths of least resistance. Mitchell (**53**) regards habits as "developed reflexes." Habits begin as volitions and after sufficient repetition take on the character of reflexes. Sherrington (**76**) points out that volitional acts may become involuntary and, conversely, involuntary reflexes may be brought within the range of consciousness.

"Familiar instances of individual acquisition of motor coördination are furnished by the cases in which short, simple movements, whether reflex or not, are by practice under volition combined into new sequences and become in time habitual in the sense that though able to be directed they no longer require concentration of attention upon them for their execution." (p. 389)

Luciani (**47**) discusses reflex action from the point of view of its genetic development and states that many "fully unconscious coördinated reflexes" were at the outset voluntary. He refers especially to such acts as walking, reading, piano playing, etc.

These statements are typical of the point of view held by a group of representative physiologists in which involuntary action estab-

lished through learning is denominated an "acquired reflex." This view receives, perhaps, its most definite expression in the following quotation from Sherrington (in Schafer's "Textbook," 75):

"On the other hand, our individual experience shows how readily volitional acts by repetition and practice ultimately become actions involving neither attention nor even consciousness,—create, in fact, habitual reflexes. The spinal reflexes may be regarded as descended from volitional acts, inherited and therefore instinctive habits of simplest order, testifying to a so-to-say primitive process akin to memory in the spinal cord." (II, p. 860)

Turning from the physiologists we find that the attitude which makes the reflex a kind of degenerated volition receives its most complete psychological expression in Wundt (91, 92). The steps in this process are volitional acts, impulsive acts, automatic acts and reflexes. The automatic act is the result of habituation and is characterized by a decrease in the conscious accompaniments. The "purposive" aspects of reflexes result from the fact that they were the voluntary acts of previous generations.

The automatizing process in connection with motor learning is stressed by other psychologists, of whom James, Bentley, and Pillsbury are examples. Habit, for James (35), is "mechanically, nothing but a reflex discharge." Pillsbury (67) indicates that in an act which has become habitual by repetition, attention is required only at the beginning; with the progress of habituation the conscious accompaniments are gradually lost, finally, in the case of certain acts, even the initiating stimulus may be unnoticed. In discussing the effect of repetition on action, Bentley (9) stresses the shrinkage and disappearance of certain processes in consciousness; the action becomes automatized, that is, "the original perception is dropping out; that a single determination is holding together the entire complete act and that it is almost entirely without mental factors." Eventually, according to Bentley, a state is reached in which the act becomes an "acquired reflex."

From the systematic texts and treatises, both in psychology and physiology, further examples might be cited describing the characteristic features of the development of a variety of involuntary action which may be regarded as being distinct from the reflex, yet carrying many features in common.

In addition to learning, the topics of attention and emotion in systematic psychology seem to have contributed discussions which are

pertinent to the development of a concept of automatic action in the field of normal psychology.

In connection with attention Pillsbury points out (66) that the movements which adapt the organism for the reception of the stimulus, as, for example, turning the head, turning the eyes, etc., are involuntary in nature. He refers to them as "reflexes of the attention." Külpe (42) and Ribot (71) make similar references. Stout (81) stresses the division of attention in the case of the skilled piano player.

The expressive movements of the face and body associated with emotional states are referred to as involuntary by Wundt (91) and Titchener (84). The classical work of Darwin on the expression of the emotions stresses the involuntary character of the muscular accompaniments.

These references, selected more or less at random, indicate the ramifications of the problem of involuntary action in so far as it applies to the particular type of movement usually designated by the term "motor automatism." In this brief survey we have limited ourselves to the field of normal psychology, and to such material as might be found in the standard textbooks.

The most striking fact in connection with the examination of these texts is the paucity of material which bears directly on the subject of involuntary action, especially in connection with automatisms. In the foregoing paragraphs the material was obtained from discussions which bear only *indirectly* on the subject in hand. With few exceptions the current textbooks in psychology contain no theoretical discussion or experimental data on automatisms as such.

Under the heading "automatic reactions" Dunlap (22) refers to actions which are characterized by a low degree of consciousness. These reactions, which were originally conscious, cover a wide variety of movements.

"The class of automatic reactions is, however, a large and varied one, ranging from the type in which ideational reactions have been reduced to the perceptual level with only occasional ideational movements, down to the type from which all consciousness has been eliminated completely—a type seldom realized." (p. 197)

These reactions, according to Dunlap, are not to be confused with reflex actions which they resemble—the important distinction being that the automatic action may at any moment revert to the conscious type.

Warren (87) refers to sensori-motor activities which are automatic and involuntary. Angell (2) discusses the tendency of ideas to produce motor changes, *i.e.*, slight involuntary movements, which are called "ideo-motor." Ideas or objects which "at once" touch off movement are discussed by both Titchener (84) and Külpe (41) as "ideo-motor" or "sensori-motor" action. The following quotation from Ladd and Woodworth (43) seems to adequately characterize this point of view:

"Certain ideas lead to certain definite movements, with which they have become associated by past experience. They may do so either with or without the full consent of the subject. When an idea leads to its appropriate movement with the full consent of the subject, we call it voluntary movement; but when the idea leads to movement, as it always tends to, while the subject's attention and intention are elsewhere directed, the movement is often named *ideo-motor*. Examples of the last are seen in involuntary whispering of what one reads or thinks, in involuntary gestures, and often, in rather an amusing way, in the movements of spectators at an athletic game or an acrobatic show, when they are much absorbed in the movements about to be executed by the performers, and unwittingly execute such movements themselves." (p. 535)

A recent criticism of the theory that every idea of movement tends to realize itself in action is found in the work of Moore (55), who restricts the application of the theory. He sums up his position as follows:

"The theory of ideo-motor action as propounded by James involves two distinct elements. One, that a kinaesthetic image must be the cause of voluntary movement. For this we found no evidence whatsoever. The second element is that the idea of a movement tends to realize itself in action. That this is universally true, is not demonstrated. It would, however, offer a satisfactory explanation of certain pathological phenomena if it were true. There is, moreover, strong evidence to show that some ideas have typical movements of *expression*, involuntary and unconscious, and common to a number of subjects.

"If, therefore, the ideo-motor theory of ideas be limited to the statement that some ideas have characteristic motor expressions, and some and perhaps all ideas of movement have a definite tendency to flow over into action, it may be looked upon as the expression of the facts as now known to psychology." (p. 330)

Data in connection with ideo-motor theory are contributed by the study of Féré (**24a**). Féré indicates that for each psychic state there is a corresponding dynamic state. He designates the automatic production of movement by ideas as " psycho-motor induction."

This brief survey of formal psychological opinion gives some indication of the part played by the automatism in systematic psychology, at least so far as it concerns the normal mind. It is, however, in connection with abnormal mental states that the chief interest in automatisms has developed. A survey of contemporary theory with reference to this field leads to a consideration of the phenomena of hysteria, multiple personality, hypnosis, somnambulism, dissociation phenomena, and the various abnormal transient states known as " fugues," " episodes," etc. A systematic invasion of these fields with a view to obtaining pertinent data is beyond the province of the present discussion. Some of the more characteristic features of automatic phenomena in abnormal mental states, as indicated by current texts and treatises, may be indicated.

One of the striking aspects of involuntary motion in abnormal states, which is frequently noted, is the fact of *dissociation*. A movement or group of movements are dissociated from the normal waking consciousness. McDougall (**49**) in a recent text in abnormal psychology introduces the chapter on "Automatisms " as follows:

" When a dissociated system manifests itself in bodily movements during the persistence of waking consciousness and normal control of the rest of the organism, it is usual to speak of the movements as automatisms." (p. 253)

These dissociated movements range all the way from muscle twitches to movements which produce intelligible vocal and written utterances, and McDougall regards them as expressing mental activity, *i.e.*, they are not the expression of purely mechanical physiological activity, but are the result of conscious activity which is analogous to the normal consciousness. McDougall believes that the automatisms of the waking state, *i.e.*, automatic writing, are of exactly the same kind as those in which the normal personality seems to be asleep, as, for example, trance states, somnambulism, hypnosis, etc. In all these states there is a " subsidiary stream of conscious mental activity " which may be described by Prince's term " co-conscious activity."

Miss Washburn in a discussion of dissociation suggests (**88**) a series of situations in which associative dispositions fail to be estab-

lished between simultaneous movement systems, that is, she attempts to answer the question: What are the conditions under which the normal associative tendencies are disrupted? Four situations are suggested as bringing about the dissociation: (1) general disturbance or shock to the organism, (2) disagreeableness of an experience, (3) concentrated attention and (4) the conditions of hypnotic trance. Under (1) is included the forgetfulness for movements which take place just preceding a strong emotional shock. In (2) she refers to the dissociation which occurs in connection with unpleasant experiences—the "complex" of Freud. In (3) we have the phenomena of automatic writing. In regard to the hypnotic state, Miss Washburn expresses the opinion that in certain individuals there exists some peculiarity of cortical organization which predisposes them to dissociation as exhibited not only in hypnosis, but in other types of dissociation.

Jastrow (**38**) finds two "functional modes" of accomplishing dissociation: (1) conscious direction is displaced, and (2) part of the muscular apparatus ordinarily under conscious direction is "wrenched away" and placed under subconscious control. Hypnotism is an example of the first and automatic writing of the second.

Another feature of automatic motor performances in dissociated states is their purposiveness and close resemblance to the movements in the normal state. The consideration of the history of reflex action in the 19th century indicates that the "purposiveness" of the reflexes of the spinal animal was the subject of a protracted debate. That discussion centered around the problem of whether or not the movements of the spinal animal, *e.g.*, in withdrawing the leg from noxious stimuli, gave evidence of the existence of a guiding intelligence, or conscious sensation. A similar problem has existed in connection with the motor automatisms appearing in dissociated states. Are these movements "mechanical reflexes" in the physiological sense? Janet (**37**) says in this connection:

"The first characteristic to be noted in all these actions is that they are not simple, mechanical reflexes; they are *intelligent acts*, which can be understood only if we admit, as present in the mind of the subject, sensations, remembrances, and even complicated reflections. The simple cataleptic attitudes of the anesthetic arm depended, as we have demonstrated, on the existence and persistence of certain very delicate muscular sensations; the adaptation of movements, however, to the nature of an object put into the hand, the obedience

to verbal suggestions, cannot be understood if there are not tactile or auditive sensations." (p. 254)

This is, of course, a restatement of the position in the earlier classical work, "*L'automatisme Psychologique*" (37).

Prince (69) is of the same opinion in regard to these phenomena, pointing out that the evidence for the existence of a "co-conscious" is as valid as the evidence for the existence of consciousness in any other individual but one's self.

Bernheim (10) points out that the phenomena of somnambulism, dreams, hypnotism, etc., always occur in a state of partial consciousness. Automatisms "sont ceux qui se realisent par un mecanisme organique, sans que l'action cerebrale psychique, volonte et intelligence consciente, intervienne dans cette realisation." Reflexes, certain habitual acts, ideo-motor phenomena belong to this category. Bernheim raises the question of whether or not the phenomena of somnambulism, hypnosis, suggestion, etc., may be explained by the doctrine of unconscious psychical action. Is it possible, for example, for one to sustain a conversation by unconscious psychical action? In such a situation automatic action plays an important part, but, in the opinion of Bernheim, it does not completely account for the behavior of the individual.

"Pour répondre à une question et parler, il faut entendre la question, il faut que cette question entendue actionne le psychisme et soit comprise, il faut que ce psychisme actionné évoque l'idée de la réponse, il faut que cette idée évoque les images acoustiques corrélatives, c'est-à-dire se traduise en parole intérieure, il faut que cette parole intérieure soit transmise par la volonté au centre bulbaire qui la réalise. Sans doute dans ces opérations complexes, comme dans toutes les opérations psychiques, nous l'avons vu, l'automatisme intervient pour une part. Mais que *tout* ce mécanisme se fasse machinalement par le seul automatisme intelligent des centres corticaux sensoriels et moteurs associés en polygone, sans collaboration de la conscience et de la volonté, même quand il y a élaboration d'idées, c'est ce que je ne puis admettre, ni comprendre." (p. 20)

In the opinion of Bernheim, the organism is so constructed that certain types of movement can take place independently of conscious control—these are automatisms—between these activities and those of the completely conscious organism exist a series of transitions rather than sharply separated categories of action.

Baudin (7) gives systematic attention to automatisms in his text-

book. He divides the automatic phenomena into normal and abnormal, classifying under the former heading motor phenomena of revery, ideo-motor action, impulsive action, motor phenomena during distraction of attention, habit, etc. Under abnormal automatisms, he includes motor phenomena of somnambulism, hypnotism, epilepsy and hysteria.

This brief survey of contemporary opinion as presented in some of the texts and monographs, indicates that under the term "motor automatism" is included a rather wide range of phenomena, both normal and abnormal. In view of their importance, there is stimulus for speculation as to the reasons for the strikingly small amount of attention given them in the current English texts. This may be due in part to the uncertainty as to what may be included under the term "automatism," and in part to the fact that the subject may be adequately covered under other headings, *e.g.*, attention, habit, and the like. A brief consideration of the historical development of the concept of automatic action, together with a review of the experimental investigations may yield information on these points.

In view of the many ramifications of the topic of automatisms, it is difficult to discover any common characteristic which may serve as a point of departure for an historical study. The automatism has in common with the reflex the characteristic of involuntariness. Involuntary action as a subject of scientific observation has a very long history, since it is with the conception of Descartes of automatic action that the modern history of involuntary action may be said to begin.

About 1760 automatisms as distinguished from reflexes, seem to have attracted the attention of Haller (29). He refers to the lack of consciousness in the "obscure perceptions" in walking, winking, and like movements. He concludes that it "is evidently false that all motions arise from the mind."

Robert Whytt in 1763 (90) in his monograph on involuntary motion—a book which is a landmark in the history of reflex action—discusses the hypothesis that the mind may control motions which are outside the field of consciousness. He notes that while walking we may carry on conversation without attending to the movements of the legs. This seems to be characteristic of a variety of voluntary motions which "are many times performed."

"We . . . acquire, through custom and habit, a faculty of performing certain motions with greater ease than we were wont to

do them, but also, in proportion as this facility is increased, we become less sensible of any share or concern the mind has in them. Thus a young player upon the harpsicord or a dancer, is, at first, very thoughtful and solicitous about every motion of his fingers, or every step he makes, while the proficients or masters of these arts perform the very same motions, not only more dexterously, and with greater agility, but almost without any reflexion or attention to what they are about." (p. 334–336)

This is a very early reference to the automatization of motor habit, and it is significant that Whytt recognizes a resemblance between these actions and the motor performances with which his monograph is primarily concerned, *i.e.*, reflexes.

In 1749 David Hartley published his "Observations on Man" (31). In this book he distinguishes between automatic acts and secondarily automatic acts. The former depend upon sensation (as opposed to voluntary motions which depend upon ideas), and the latter depend upon the "most diminutive sensation, ideas and motions, such as the mind scarce regards." Repetition of motion tends to eliminate the action of the will and make the act automatic.

George Prochaska in the "Dissertation" (70) published in 1784 regards the movements of the heart, oesophagus, stomach and intestinal canal as being completely automatic throughout life. In the case of voluntary muscles, Prochaska notes that under certain abnormal conditions the control of the mind is renounced "as is seen in hysterical, epileptic, or infantile convulsions, or in those affected with St. Vitus's dance; and these movements, although performed by muscles designated voluntary, can only be termed automatic."

"The raising of the hand and the application of it to the head in apoplexy belong also to the class of automatic movements, also the turning of the body in sleep, and partly even somnambulism itself, which, however, it would seem is partly also to be ascribed to obscure sensations and volitions which the mind instantly forgets." (Chapter V, Sec. IV)

Sir Gilbert Blane in one of the Croonian Lectures (15) in 1788 distinguished between habit and unlearned automatisms. Of the former, he said:

"It is the nature of a voluntary muscle to perform any motion with greater ease, the more frequently it is repeated, and to act most readily with those muscles, or in company with those sensations with which it has been used to combine its action either at once or in

succession. This is the foundation of habit, and is the principle by which all the practical attainments of man acquire facility and perfection." (p. 263)

At the beginning of the 19th century, interest in involuntary phenomena was tremendously accelerated by the discoveries of Bell, and Magendie in connection with the function of the spinal nerves, and by the work of Marshall Hall on reflex action. The work of the latter, like that of Whytt in the preceding century, may be regarded as one of the guide posts in the history of involuntary action. Hall was concerned primarily, however, with the establishment of a physiological concept of reflex action, and in his work it is possible to find but little reference to anything corresponding to automatisms in the modern sense. He regarded the action of the sphincters as being "automatic," and discussed the involuntary motor phenomena in epilepsy. Hall did much to establish the concept of a mechanically functioning reflex arc, but seems to have been but little concerned with acquired motor automatisms.

Johannes Müller in the "*Handbuch*" (**58**) makes reference to a variety of movements which he terms "mitbewegungen" which possess certain characteristics of automatisms. In this class of movements, Müller referred to any movement which is called forth or associated with another movement. In endeavoring to move the ear, for example, one also moves the facial musculature. The ability to eliminate these accessory movements improves with practice. The piano player, for instance, learns to eliminate the movement of certain muscles.

The rigid distinction of Hall, Müller and other physiologists of the early 19th century between voluntary action on the one hand, and involuntary (reflex) action on the other met with opposition on the part of Griesinger (**26**) in 1843 and Laycock (**45**) in 1845. These investigators, independently of each other, suggested the possibility of reflex action taking place through the brain, *i.e.*, a "psychical reflex." According to Griesinger the psychical reflex includes the action of the brain in connection with "ideas of effort" (Vorstellungen in Strebungen) which may be either conscious or unconscious. Laycock extends the "laws of reflex action" to the brain, and attempts to find in the cerebral nerves a basis for action which is strictly analogous to the reflex action taking place through the spinal nerves. In the case of hydrophobia Laycock finds an example.

"I have stated that the idea of water, whether obtained through

the eye or ear, will excite the hydrophobic gasp and convulsions; it will also excite a conservative act, the patient, when water is presented to him, is horrified, and immediately attempts to remove it. This movement is strictly involuntary, and not the result of sensation; the water is repelled from the lips with a violent spasmodic jerk, and often in spite of the urgent volitional attempts of the patient to the contrary, just as the hand is snatched away from a spark of fire, or the headless frog leaps from the needle." (p. 302.)

The idea of psychical reflex action was elaborated by Carpenter (16) under the term "unconscious cerebration." He says:

"This 'Unconscious Cerebration' . . . is the precise parallel, in the higher sphere of cerebral or mental activity, to the movements of our limbs, and to the direction of those movements through our visual sense, which we *put in train* volitionally when we set out on some habitually-repeated walk, but which then proceed not only *automatically,* but *unconsciously,* so long as our attention continues to be uninterruptedly diverted from them." (p. 515)

This process Carpenter also calls "ideo-motor" action, a principle which is extended to cover the acts of the somnambulist, the hypnotized subject, the phenomena obtained with the use of the planchette or ouija board, the solving of problems during sleep and the like.

Richet developed a theory of "psychical reflexes" in a paper published in 1888 (72). The essential elements in a definition of reflex action are: (1) its involuntary character, (2) its immediacy, and (3) the necessity of peripheral excitation. Richet distinguishes between "conscience" and "connaissance," the latter term being applied to the mental experience which accompanies the psychical reflex. This experience is vague, fleeting, indefinite and is characteristic of the *psychical* reflex but not of the *simple* reflex. In the simple reflex it is the *amount* of stimulus, while in the psychical reflex it is the *quality* of the stimulus which determined the motor response. When there is some appreciation of the exciting stimulus—as in the case of the ability to partially inhibit the reflex wink when a pretended blow is aimed at the eye—Richet terms it a psychical reflex. Psychical reflexes may be innate or acquired. The latter include habits and account in a large measure for the differences in response between individuals, *i.e.,* it is the individual's particular body of acquired psychical reflexes which differentiates him from others with the same hereditary equipment.

Reflex action was the principle used in the explanation of hypnotic

phenomena offered by Heidenhain (32) in 1880. Heidenhain regards the hypnotized person as behaving like an "imitating automaton." The movements in such a subject are described as "imitation automatisms," "speech automatisms" and "command automatisms"—the latter being actions carried out at the command of the operator. These automatisms for Heidenhain are merely reflex acts the stimuli for which are the movements (visual stimuli) or commands (auditory stimuli) of the hypnotist. Consciousness and will are not involved; the hypnotized subject, in fact, might be regarded as a spinal animal.

This point of view was, as has been indicated above, in opposition to that of Janet as indicated in the classic work of 1889 (36), in which the automatisms of the subconscious states were regarded as giving evidence of the existence of a secondary consciousness.

Janet in this work makes clear what he means by unconscious action: it is an action which has all the psychical accompaniments except that it is wholly ignored by the person who executes it. Actions which are immediately forgotten are not included under this definition.

Further consideration of the theories of automatic action in connection with abnormal states would lead to an analysis of the phenomena of hysteria, somnambulism, hypnotism, suggestion, etc., as they are related to theories of unconscious action. These are topics beyond the limits of the present section: Reference to the work of Moll (54), Bernheim (11), Gurney (27), Despine (21), Binet (12), Binet and Fere (13, 14), F. Myers (60, 61), Beaunis (8a) and others indicates the extent of this field as well as its controversial nature.

The French psychologists and physiologists were primarily concerned with automatisms as they appeared in various abnormal states. In contrast, however, the interest in the subject indicated by the work of the English investigators—Whytt, Hartley, Blane, and especially, Carpenter—emphasized the relation of automatic action to certain phases of motor habit. This is evident in the systematic work of Sully (82), Maudsley (50, 51), Bain (3), and the American psychologist, Baldwin (5). Sully notes the mechanical nature of habitual action and, following Hartley, he calls it "secondarily automatic." Movements of this type are unconscious and closely resemble reflex action. Maudsley states that learned acts may become "secondarily automatic" and take place without consciousness; walking, for example, is an act of this type. Bain divides reflexes

into excito-motor, following Marshall Hall, and sensori-motor. The latter may involve consciousness, but do not include movements of "emotional diffusion." These movements accompany the expression of emotion, and are involuntary but not unconscious. Baldwin, under the caption "Motor Value of the Subconscious," discusses those "motor phenomena which fall below the threshold of conscious reaction." Action during "absent-mindedness," post-hypnotic suggestion, etc., are discussed under this heading. Baldwin notes that a "nervous man will arrange his necktie or stroke his mustache fifty times a day without 'knowing it,' and all of us have our little motor habits, which we are conscious of but do not observe."

The experimental attack on the problems of motor automatisms seems to have been undertaken in the latter part of the 19th century. Jastrow (**39**) in 1892 devised apparatus (the automatograph) for the laboratory study of involuntary movements. This instrument consists of a writing needle attached to a glass frame which is supported by steel balls. The hands of the subject rest on the glass which responds to very slight movements. Jastrow noted that involuntary movements tended to follow the direction of attention; the hand tended to follow the direction of the eye. In a later study (**40**) Jastrow registered the sway of the body by a writing needle attached to the head. He found that the head tended to follow the direction of attention.

In 1895 Lindley (**46**) studied the automatic motor phenomena of mental effort, using the questionnaire method. Six hundred and sixty-two cases are represented in his data. His results indicated that (1) automatisms of the accessory muscles of the face, head, fingers, feet, etc., were the most frequent, (2) they tend to disappear with age, and (3) they tend to increase with fatigue. Lindley classified the automatisms into (1) those due to sympathy and imitation, (2) excitatory automatisms, *i.e.*, those which seem to facilitate brain work provided they do not distract attention, (3) those automatisms which precede concentrated attention, and (4) automatisms of posture.

Newbold (**65**) in 1895 discussed the problem of processes which take place in the brain without being represented in consciousness. He especially referred to the phenomena of crystal gazing and hallucinations on the sensory side, and to automatic writing on the motor. His experiments were in connection with crystal gazing (sensory automatism) and automatic writing. Newbold did not

believe in the hypothesis of a submerged personality, but explained the automatic phenomena as ideas obtaining motor expression.

The experiments of Solomons and Stein (79) in 1869 approached the problem of normal motor automatisms. The object of their experiments was to investigate the limits of motor automatisms in the normal individual, and to determine if possible whether or not the performances of the hysteric and "split" personalities were allied to the automatic acts of the normal individual. They listed four elements in the unconscious movements of secondary personalities: "(1) general tendency to movement without conscious motor impulse, (2) tendency of an idea in the mind to go over into a movement involuntarily and unconsciously, (3) tendency of a sensory current to pass over into a motor reaction subconsciously, (4) unconscious exercise of memory and invention." The investigators attacked the first problem by the use of the planchette—a form of Jastrow's automatograph—which the subject operated while reading a novel. The subject performed various movements under these conditions. Under (2) the subject was given a pencil which he kept continuously moving while engaged in reading a novel. The writing soon became automatic, and frequently words were written which the subject had just read. In the third group of experiments, the subject was required to write from dictation while engaged in reading. Whenever the attention was sufficiently distracted in this experiment, "real" automatisms appeared, that is, the writing from dictation went on below the level of consciousness. In these cases the subject was unable to recall a single word written. The experimenters discuss the problem of consciousness without memory vs. "real" unconsciousness in this connection.

"The consciousness without memory seems to *approach as its limit*, simply a condition in which the subject has not the faintest inkling of what he has written, but feels quite sure that he has been writing." (p. 501)

This is a state of "real" unconsciousness and closely resembles the phenomena of hysteria. A similar type of experiment was performed in connection with automatic reading in which the subject reads aloud while being read to. Attention of the subject is given to the material being read to him, and after practice is able to continue his own reading in an entirely automatic fashion. Unconscious invention (4) was experimented upon by means of automatic writing.

The experimenters concluded that a large number of acts or-

dinarily called intelligent, such as reading, writing, etc., are in reality automatisms. In regard to the problem of whether or not there was an analogy between these normal automatisms and the phenomena of secondary personalities, the experimenters make the following comment:

"It will be remembered that these phenomena occurred in us whenever the *attention* was removed from certain classes of sensations. Our problem was to get sufficient control of the attention to effect this removal of attention. In hysteria this removal of attention is effected by the anaesthesias of the subject. We *would* not, the histerique *can* not, attend to these sensations. Whatever else hysteria may be then, this, at least, seems most probable. It is a *disease* of the *attention*. An hysterical anaesthesia or paralysis is simply an inability to attend to sensations from this part." (p. 510–511)

Stein (**80**) in later series of experiments was able to "teach" a normal subject certain automatic movements when his attention was distracted. The movements "taught" were figure eights, m's, curves, etc.

Solomons (**78**) in 1899 continued the study of motor automatisms using the reaction method. In these experiments, the subjects were required to react to auditory stimuli while reading entertaining literature. The usual chronoscopic technique was employed. Most of the subjects were able after practice to react automatically. The reaction times ranged from over 290 sigma to less than 100 sigma. The experimenter divided the reactions into four groups. (1) Reactions above 290 sigma which contain an element of will. (2) In reactions below 290 sigma nothing is left of the motor impulse except the feeling of personal activity. These correspond to the "sensory" reaction type. (3) These are reactions from 175 to about 225 sigma and are characterized by a prominence of the "reaction feeling." The fourth group may be "extra-cortical," although the author points out that lack of knowledge of the finer anatomy of the sensori-motor paths makes generalizations in this field difficult.

The work of Solomons and Stein was repeated and amplified by Tucker (**85**) at Stanford University. A large number of subjects were used by this investigator and the automatograph of Jastrow was employed. The conclusions were (1) there is a physiological tendency for the hands to move inward toward the "median plane of the body," (2) ideas of motion tend to cau[illegible] involuntary move-

ments, (3) involuntary muscular movements tend to imitate moving stimuli.

Allonnes (1) in 1905 devised apparatus with which the subject graphically recorded involuntary movements while letters and digits were presented to him. Involuntary movements were also recorded while words, phrases, and arithmetical operations were presented for his consideration. The results of this experiment were of the usual sort and seem to be in general agreement with those of Solomons and Stein.

Curiously enough, there has been but little attention given to the experimental phases of the subject of motor automatisms since the group of experiments which have just been reported. This in spite of the enthusiasm of Myers (62) for experimental data on the problem of the subconscious expressed in his review of the paper of Solomons and Stein. The report of experiments on automatic writing are found in the work of Myers (63, 64) in connection with the phenomena of the seance room, and in the papers of Miss Downey (23, 24). The paper of Wells in 1916 (89) on typewriting may be mentioned as offering some data on motor performances in this particular field. To advance further into the field of motor learning would be unwarranted.

The experiments of Lashley (45) in 1921 on cerebral functions bear directly on the problem of automatisms. He was concerned with the presence or absence of cortical representation in the case of automatized habits in rats. The habit established was the discrimination between light and dark alleys in the Yerkes discrimination box. Lashley notes that no data are available as to the amount of practice necessary to thoroughly automatize a habit in man. Thirteen hundred repetitions were used in the case of the rats. The visual area of the cortex was destroyed and the animals tested for retention, with the following results:

"The functional activity of the visual cortex is still necessary to the performance of the habit and there is no indication that subcortical nuclei have taken over any part of the reaction, even sufficient to facilitate relearning. The cerebral area functional in learning seems to retain the same function after prolonged training." (p. 465)

That is, destruction of the visual cortex resulted in a loss of the visual-motor automatism. This seems to answer the question as to whether "lower centers" take over the functions of the higher centers when a habit becomes automatized. Lashley comments as follows:

"If long practiced habits are not reduced to subcortical levels what is the neurological basis of automatization? The musician may not speak when first learning a difficult movement but later his verbal reactions are dissociated from the manual coördinations so that the two processes may go on simultaneously. It is this capacity to function without exciting reaction systems other than those directly concerned with its performance that characterizes the automatic habit. Such a condition might be brought about by blocking cerebral associative connections, and this seems to be the only alternative to reduction to subcortical levels. An analogous situation is presented by the differentiation of the conditioned reflex to a specific stimulus. Whether the confining of impulses to a single path is the result merely of repetition or of some active inhibitory or blocking process cannot be decided from existing evidence." (p. 467–468)

Lashley suggests that the mechanisms of conflict and repression in the Freudian sense may have something in common with the processes of automatization.

Maxwell (52) had attempted in 1916 to work out a neurological schema which would serve as a basis for automatic writing. He noted that these movements are combined and coördinated, and that they express coördinated thought. His neurological schema was based on an hierarchical arrangement of centers in an attempt to account for the observed levels of complexity.

Although but little has been done in the last fifteen years in the way of experimental work, the theoretical interest in the phenomena of the subconscious and unconscious has resulted in a large literature. It is not the present purpose to review this material, but reference may be made to certain papers which bear more or less directly on automatisms. The paper of Hart (30) contains an excellent review of the theories of the subconscious, especially in connection with the problem of the existence of a subconscious personality. With reference to this problem Münsterberg (59) points out that there are three types of theories: (1) the subconscious is regarded as a complete psychical system—a personality. This is the theory of the layman. (2) The subconscious is psychical in nature but not a system—merely split off material. This is the medical view. (3) The subconscious is not psychical, but is merely brain process. This is the orthodox psychological view. The view of Prince (68) which regards the evidence as indicating an existence of psychical activity, is expressed in the same symposium of which the paper of Münsterberg is a part.

Delacroix (20) in a theoretical article discusses the relationship of imitation to automatic movement. He distinguishes between voluntary and involuntary imitation, the latter being in the nature of an automatism.

From time to time there have appeared in the psychiatric and spiritistic literature descriptions of particular types of motor automatisms. Typical of these reports is that of James (34) and Cory (18) in connection with automatic drawing, and that of Salmon (74) and Bancroft (6) in connection with clinical observations. The automatic phenomena in epilepsy and sleep walking (ambulatory automatisms) are presented by Singer (77), McCarthy (48) and Courtney (19). The definition of these ambulatory acts as given by McCarthy is interesting.

"It is that condition in which an individual consciously or unconsciously performs more or less complex ambulatory acts over which he has no control. These occur in hysteria, epilepsy and neurasthenia."

Recently an interesting clinical application of the technique of automatic writing has been made by Mühl (57) who used it as a means of obtaining information regarding the subconscious factors underlying personality. Mühl includes in this article a discussion of the levels of consciousness. These are (1) focus of attention, (2) consciousness, (3) fringe of awareness, (4) paraconscious, (5) personal unconscious, (6) genetic unconscious. These levels are dissociated in abnormal states, and the material obtained by means of automatic writing is from the paraconscious and unconscious.

The importance of automatisms in the phenomena of the séance room and in connection with the states of trance, "possession," thought transference, etc., have been attested by Meyers and others. Coover (17, pp. 143, 152) in his monograph on psychical research stresses its importance as an explanatory principle in interpreting subjective experiences and muscular manifestations, which are, in reality, hallucinations[1] and motor automatisms.

A recent consideration of automatisms in connection with education is found in the brief note of Mudge (56) who found that a large proportion of women students mentioned some form of motor automatism accompanying study. Mudge suggests that certain muscles strains are associated with certain states of attention; such states of attention being impossible without the accompanying posture

[1] The subject of sensory automatisms, subliminal impression, etc., are outside the field of the present study. They have, however, important theoretical and experimental connection with it.

or movement. Mudge's data are too few to warrant any conclusions, but they contain a significant suggestion for experimental work.[2]

An adequate summary of the work reviewed would be difficult both because of the complexity and diversity of the materials involved and the extent of territory covered by the topic. The phenomena of motor automatism appear both in normal and abnormal mental functioning. In the field of normal psychology they appear in connection with attention, habit formation and ideo-motor activity. In the field of abnormal psychology these phenomena appear in practically every type of atypical mental state or function. From the point of view of psychological theory, there are found many points of contact: the problem of attention, the nature of long continued practice, the problem of ideo-motor action. In the abnormal field such broad problems as the theory of the subconscious, the nature of hypnotism and multiple personality are involved. There seems to be an essential identity between automatisms as found in normal life, *i.e.*, automatized habits, the phenomena of "absent-mindedness," etc., and the motor mechanisms observed in the various abnormal states. Automatisms may be considered as a sort of bridge—one of many—between the normal and abnormal. The importance of this bridge makes more apparent the lack of systematic attention which the problem has received at the hands of experimental psychology.

Motor automatisms bear an important relationship to the theory of reflex action. Historically this is evidenced by the early concern with "cerebral reflexes" and "psychical reflexes." The common characteristics of involuntariness, lack of central control, and low degree of conscious accompaniments seem to be more than mere surface resemblances. The work of Lashley would seem, however, to bring negative evidence to bear upon this point, unless the reflex arc is assumed to take place through the cortex. Our knowledge both physiological and psychological of reflexes is much more complete than is our knowledge of automatized habits. Further experimental analysis of the latter is necessary before these problems can approach solution.

[2] There seems to be a suggestion here of Titchener's context theory of meaning.

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SLEEP

BY H. M. JOHNSON, T. H. SWAN AND G. E. WEIGAND
Mellon Institute of Industrial Research, University of Pittsburgh

The present review is confined to a few of the more important of the recent studies of psychological aspects of sleep, and does not aim at completeness. The topics illustrated are (1) method and technique employed in attempts to study "the depth of sleep"; (2) experimental studies of insomnia; (3) certain physiological variations; and (4) hypotheses.

Depth of Sleep. Numerous students have been interested in the question of the "rate" or "depth" of sleep, and have made measurements upon different variables which might be so named. The method employed in the early studies of Kohlschütter, Michelson, Howell, and others, was to measure either the intensity which a stimulus of a given mode must have in order to awaken the sleeper; or else the number of times a constant stimulus had to be applied in order to produce that effect. The criteria of "awakening" varied according to the experimenter. Inasmuch as the method of experimentation interfered with the behavior of the sleeper it was necessary to limit the number of interruptions on a single night. Since a habit of expectation can be set up if the disturbances are made at nearly the same hour of different nights, they had to be distributed accordingly. Since there is reason to expect variations from non-experimental sources, it is necessary, in order that they should average out, to continue the experiment through a very large number of nights, on a single subject. Meanwhile, there is a chance of setting up a habit of "sleep" which may differ greatly from the one which prevailed at the beginning of the experiment. Karger (10), Richter (20) and others have commented upon this fact, and have sought other means of approach to the problem.

Szymanski (24), interested in the distribution of periods of activity and "relative" rest, rather than of "sleep," constructed some nine different types of suspension for cages, beds, etc., which permitted recording instruments to be attached to the latter, rather than to the animal; and which served to register kymographically at least the more important changes of posture and position, along with a time-

record. This apparatus enabled him to obtain typical records, in some instances over a period of many months, of the distribution of activity of animals as various as snails, fish, snakes, birds, rodents, dogs and children. Karger (10) reports that Naegele devised an improvement in the mounting. Karger himself (11) placed the feet of the bed on rubber pads, and attached the bed-frame to a sort of seismograph, made from parts of a recording aneroid barometer. Two of the present writers (J. and W.), before learning of Szymanski's results, tried a number of mountings of beds for use in studies of the sleep of adults, and chose a system of pendular supports—one under each foot of the bed; each pendulum being swung from a point-support, rather than from a knife-edge; the foot of the bed engaging the bottom of the pendulum in a similar manner. This mounting gives sufficient sensitivity without the necessity of magnifying the movement on the record.

With respect to all these devices, we may remark, that none of them will give a faithful record of the movement as to either magnitude or time; since all of them are subject to periods of their own; and since, moreover, the record of two movements opposite in direction but occurring close together in time, is a function of the algebraic sum of the directed forces, while "activity" is a function of the sum of their absolute magnitudes. It is doubtful, however, whether a faithful record would receive elaborate analysis; or whether the analysis would lend itself to ready interpretation, if made in our present state of knowledge of the factors involved.

Karger reports that children usually exhibit little activity during the earlier hours of the night unless they are psychopathic in type. In this respect his results bear a resemblance to the results of Kohlschütter, Michelson, and Howell. Our own results, obtained on eleven healthy young men, do not, in general, meet this expectation; some subjects tending to rest more during the first half of the night; others during the last half; others during the middle; and still others during the first and the last quarters. Some of the individual differences are characteristic.

Karger (11) found that nocturnal activity in children was increased by ingestion of caffein and atropine; by study during the evening; by the presence of another child in the same sleeping-room; by suggestion (the patient was told that another child was to be operated upon during the night; and that he must not be frightened if he heard it scream); etc. Fever, in some cases produced "motionless sleep," in other cases heightened activity. Some

soporifics such as luminal did not show their full effect until the second night after they were taken. The results so far as they go are very suggestive, although it is not clear that satisfactory norms were obtained by which to compare the results of experimental disturbances. Our own results show important variations in the number of interruptions of rest from night to night, on the same subjects; although if enough records be taken, the differences among the averages of the several individuals are statistically reliable; so much so, indeed, as to suggest that the measurement may prove to be an exceedingly important datum, in case one can learn to interpret it. An editorial writer (in the current volume of the *J. of the Amer. Med. Assn.*) adds the criticism that Karger used hospital patients as subjects, rather than perfectly healthy children.

The question arises, what relationship has a time-record of postural activity to other "criteria of sleep"? Here we must remark that "sleep," so far, is a term which is not at all well defined; and that usually, to call a certain variable a "criterion" of sleep, amounts to nothing more than giving a new name to the variable. It would make for clarity, in most cases, if the different variables were consistently called by their own names and not labeled "criteria" at all. This comment noted, we may answer the question: we do not know. To provide a better answer, one must resort to the laborious method of making simultaneous records of the variables to be compared; obtain enough of them to justify intensive mathematical treatment; and subject them to that treatment. So far no published studies appear to have aimed at satisfaction of this requirement. There are, however, some reports of incidental observations which are not without interest.

Mosso and Shepard, individually, observed a change in the character of respiration at the instant of falling asleep; in that the abdominal excursions diminished, while the thoracic movements increased, in amplitude. It is not perfectly clear what other criteria of "falling asleep" they used. Karger suggests that cessation of postural activity tends to accompany this change, although his description is not perfectly clear. Reed and Kleitman (19) found that this change in the character of respiration was by no means a concomitant of "sleep"; their "criterion" of the waking state being their success in gaining a response, namely, of pressing a key left in the subject's hand, when the experimenter actuated a telegraph-sounder placed at the head of the bed.

Karger (10) asserts, apparently on the authority of Peiper (18),

whose paper we have not seen, that the so-called psychogalvanic reflex is abolished in sleep. Richter (20), in this connection, has called attention to a striking difference between the temporal variations in body-resistance, which depend on the surfaces to which the electrodes are applied. If the electrodes be applied to the palms of the two hands the resistance shows an increase, in a maximum ratio of some 16:1, during a period in which the subject was said to be "asleep." During the same period, with the electrodes applied to the backs of the hands, no significant changes occurred. In the case of a patient in a catatonic stupor, during several hours, no significant changes in the resistance in the path from palm to palm appeared; whereas, during the same period, the resistance of the path from the back of one hand to the back of the other varied through an extreme range of 9:1. From experiments with atropine, which greatly increased the resistance of the path from palm to palm, without affecting the resistance of the path from back to back; and from observations of the results of cutting the left sciatic nerve in a monkey, which enormously increased the resistance of the path between one sole and the other, without affecting the resistance between the path between the backs of the two feet, Richter had concluded that the effect was due to the inhibition of nervous current to the sweat-glands, the latter being numerous in the skin of the palms and soles, and scarce in the skin of the backs of the hands and feet. That the variation is dependent on conditions in the skin, he demonstrated by the fact that two small punctures in the skin, presumably at the surfaces of contact with the electrodes, sufficed to reduce the resistance "practically to zero." (The actual figure is 15,000 ohms, as compared with an initial value of 500,000.)

Richter asserts that one can diagnose the depth of sleep by this method. In the opinion of the reviewers more evidence is necessary. His claim probably reduces to something like this: that the experimenter, watching the galvanometer or its record, but not the sleeper, would agree reasonably well with an observer watching the sleeper but not the galvanometer, as to the times at which they would say the patient was "asleep."

Karger mentions a clinical observation that the rate of perspiration in children often increases greatly from 1.5 to two hours after they fall asleep—*i.e.*, in the interval during which he judges their sleep to be the deepest. On such a basis one would expect the resistance between two regions on the skin which abound in sweat-glands to diminish at such times, rather than increase. In our present state of

knowledge this observation appears to be not fully consistent with Richter's findings.

We may summarize the above by saying that a number of discrete phenomena, each of which is interesting enough in itself, have been indiscriminately labeled indications of the depth of sleep; the latter being left undefined. It remains to show the degree of intercorrelation among the several variables; pending which finding it is doubtless better to treat them separately.

Experiments on Deprivation of Sleep. An interest in the effects of experimentally induced insomnia has become very widespread in recent years. Popular interest has fallen but little short of scientific interest, to judge from the newspaper accounts of the experiments.

Smith (23) acted as subject in her own study. Her procedure is essentially as follows: "Normally," she writes, "*I sleep about eight hours* and do not suffer from insomnia."[1] (Italics ours.) She practiced daily after "normal nights" in certain tasks until she had established satisfactory norms; then, kept three vigils in succession, as follows: on the night of the first vigil she "slept" 1½ hours; of the second, 3½ hours; of the third, 5½ hours. She continued the tests at the usual hours on the days following these vigils and for days thereafter, the daily scores being noted; and on occasion she introduced additional vigils to ascertain the change which resulted in the prevailing course of recovery.

The tests she employed were the following:

1. The "dotting test" of McDougall, scored in terms of errors and of accuracy in aiming.

2. Recall of a sequence of forty words, read once. The "principles of association" between each member of the list and its successor varied, but according to the author some principle was always active. A segment of one list is, "mountain, plain, ugly, beauty, Venus, Greece, oil, smooth, rough, rude, cultivated, refined, sugar, sweet, salt, sea," etc. If any word was not recalled correctly within 10 seconds, the failure counted as an error; if incorrectly reproduced but corrected within 10 seconds, half an error was noted.

3. The "windmill" illusion of reversible perspective of McDougall. The number of changes per minute were noted.

[1] We take this assertion as meaning essentially that ordinarily she spends about eight hours nightly in bed, during which time little activity occurs of which she can give a verbal account next day. The distinction has not high importance for her report, but might acquire such importance for a paper given to a discussion of her results.

4. A tapping test, given four times at each sitting for 15 seconds at a time.

5. Learning and relearning of nonsense-syllables.

The results obtained on tests (1) and (2) singly and combined are of especial interest.

On the dotting test (No. 1) the number of errors diminished appreciably on the days following the three vigils, by an amount which exceeded the normal range of variation. Immediately thereafter the number of errors increased to 2.5 times the normal, the performance then gradually tending to return toward the normal (except for one day) for 9 days, when two additional vigils restored it. (During a part of the intervening period the subject increased her ration of "sleep.")

A similar effect was found in test (2); and except that depression was delayed, an effect like the first, except as to degree, in the scores made in the two tasks when performed simultaneously.

In test (3), the windmill illusion, the average number of reversals a minute following the vigils, was increased from a normal of seven to "between 13 and 40 for several days." The normal variability is not given. The author discounts the result on the ground of "suggestion."

In tapping (test 4) she found "a gradual deterioration" in the days following the vigils, but the records were incomplete. In the learning of lists of twelve nonsense-syllables the average number of repetitions, normally seven, was increased to 13, and the average number of repetitions required for relearning, normally three, was increased to seven, in the first phase of fatigue: namely, the period of depression following the last vigil.

We shall postpone discussion of these results for the immediate present. Meanwhile we shall mention two other sets of results which are concordant with Smith's findings.

Wyatt and Fraser (**26**) studied for three weeks the behavior of four women engaged in the operation of stamping lids for cigaret-tins out of sheet tin, by means of a press operated mechanically by a pedal. The basis of pay was piecework; the hours of labor being 8 A.M. to 1 P.M. and 2 P.M. to 6 P.M. except that on Fridays work ceased at 5 P.M. and on Saturdays at noon.

Three variables were measured: namely, (1) output, in number of pieces per half hour; (2) duration of useful labor within each half hour, obtained by means of a stopwatch; and (3) output per half hour of useful labor. It was found that the output increased

steadily for the first two and one-half hours, to a point 16.7 per cent above the initial value.

We do not know whether, during this time the reserve supply of fuel stored *within* the receptor, conductor and effector cells was being depleted more rapidly than it was being renewed or not; neither do we know whether toxic waste-products of activity were being accumulated *within* the active cells more rapidly than they were being removed. In both cases, the more conservative assumption—if one must make any assumption at all—is that they were. If so, we have given what Smith, seemingly, found: performance increasing while "fatigue," in a physiological sense, was also increasing. The maximum output in Wyatt and Fraser's study was found between 4:30 and 5 P.M.—after 7½ hours of hard labor. There are a number of other interesting data in Wyatt and Fraser's study, but we must postpone consideration to a later article.

We now take the liberty of adding some results of a study made by two of the present authors but not yet published. Twelve healthy young men were required to spend 7¾ hours a night in bed five nights weekly for a period of several months. The sleeping-room was adequately heated and ventilated and kept clean and in a sanitary condition; the bedding equipment, in our opinion, was somewhere near the second best type on the market, and far superior to the kind usually found. On the experimental nights, a half hour before the reactors retired, they were subjected to a substitution-test, lasting for 15 minutes: an equivalent test was given the next morning beginning about a half hour after they arose. Two forms of test were used, but the two sets of results are almost identical. An effect of daily practice being corrected for, it appeared that for the group as a whole performance at night is of the order of 6 per cent better than performance in the morning, the difference being about ten times its probable error in one test and over twenty times its probable error in the other, the difference between the ratios being determined chiefly by the difference in the number of experiments. Of the twelve subjects employed, ten showed reliably a tendency in the same direction; one showed reliably a contrary tendency; while one was indifferent.

Two explanations of such effects as the above may be selected from the multitude that have been offered.

(1) Fatigue-toxins, in sufficiently small concentrations, act as exciting agents, or at least as selective depressants. In this respect

they resemble alcohol, the opiates, ether, chloroform and chloral hydrate.

(2) Muscular tension, acting on intra-muscular receptors, provides the major portion of the nervous current which is utilized in effecting response. Its maintenance depends upon the repeated activity of circular reaction-arcs. When interrupted—as in sleep—considerable time must be spent in repeated activity to restore a degree of tonus comparable to the original one. It is not improbable that relaxation is, itself, a positive condition requiring continuous, or repeated, innervation to maintain it; so that like many other phenomena of behavior it is subject to the "laws" of habit.

The two hypotheses are mutually compatible, although the second does not necessarily imply the first. The first is the more readily capable of experimental attack, although so far, great success has not attended the attempts to isolate the supposed fatigue-toxins and to investigate their properties. An adequate test of the latter would require a satisfactory method of measuring muscular tonus; such a method has yet to be developed.

Scientific expectations regarding effects of long periods of insomnia are somewhat set by such a report as that of de Manacéïne (16) and by those of other students whose results confirm hers. In the article cited she quotes Hammond, a medical writer, who remarked that *absolute* insomnia is extremely rare; nine or ten days being sufficient to cause death; that "nervous disorders" rarely involve complete insomnia but only short and broken periods of sleep. Renaudin, also cited by de Manacéïne, had noted that partial insomnia is often followed by grave derangements of behavior.

De Manacéïne kept 10 nursing puppies awake for varying periods—two until they died, at the end of 92 and 143 hours respectively; the rest for periods between 96 and 120 hours. Although they were artificially warmed and fed, all were dead within a week, the younger animals being the least resistant. The body-temperature dropped within 24 hours by amounts ranging between 0.5° and 0.9° C., then more rapidly to 4°–5.8° C. below normal at the end. Reflexes showed a certain periodicity of abatement and restoration, the pupillary reflex being among them; but different systems of reactive organs might present different phases of the rhythm at a given time. Red blood corpuscles declined from 5 to 3 million per cubic millimeter after 48 hours and to 2 million by 110 hours. In the last two to three days before death, there is an apparent increase in the number of red corpuscles and also in

hemoglobin; but this effect is due to decrease in water-content of the blood, as the animals by this time refused to drink while they continued to excrete. The proportion of leucocytes increased progressively during the experiment. Histological study of the brain showed "fatty degeneration" in ganglion cells; engorgement of blood vessels with leucocytes; and capillary hemorrhages, especially about the optic nerve and in the optic lobe. The brain preserved its normal weight.

Crile (3) shows plates of sections of the central nervous system of an animal kept awake for 72 hours. The chromophilic substance is depleted in quantity and scattered instead of being aggregated in islands; the outer walls of many nuclei and of the cell-bodies themselves are disrupted. These effects are strikingly like those found in the brain cells of animals which had died of starvation, of muscular exhaustion, of narcosis and of specific disease. While certain distinguished anatomists indicate the need of caution in the interpretation of histological data, the findings are not easily disregarded.

During the past four years several studies have been made in America of the effects of deprivation of sleep. Inasmuch as the results as well as the psychological methods employed have a good deal in common, we shall consider them together:

In the study of Robinson and Herrmann (21) three graduate students—all of whom were men between the ages of 20 and 22—were employed as subjects. They were deprived of sleep for an interval of 60 to 65 hours. Preparatory to the insomnia they were practiced in the tests employed for 13 to 26 days, the time varying according to the subject and test; so that a sort of norm was established. The daily tests were repeated for four to six days after the period of insomnia. The tests were: strength of grip, speed of tapping, accuracy in aiming, speed and accuracy in reading alphabetical letters one by one; and speed and accuracy in "mental" multiplication.

In a second experiment Robinson and Robinson (22) subjected a group of college students, consisting of 3 women and 22 men, to test by three of the various forms of the army alpha "intelligence-test"—so-called. These reactors, after taking the first test, were prevented from sleeping until the second test was given the next day;[2] after which they were then released from control, but given a third

[2] Two of the group reported 15-minute cat-naps during a class-period before the second test was given.

test on the day following. A second group, consisting of 2 women and 37 men, who lived as usual during this period, was used as a control.

Laslett (15) employed six healthy young men as reactors. He first trained them for periods ranging between 10 days for one test and 4 for another; then deprived them of sleep for about 50 hours, testing them twice during that interval, and not continuing the experiment farther. His tests were three: namely, (1) recall of digits in three 12-place numbers; (2) speed and accuracy in a substitution test, the time allowed for which was limited to 2.25 minutes; and (3) the weighted score in the Pintner-Renshaw test of analogies.

Kleitman (12), as a supplement to an excellent physiological study, employed two tests: letter-naming and mental arithmetic. He used six subjects, all healthy young men, and deprived them of sleep for 40 to 115 hours. This duration was not absolute; for, despite all precautions, his subjects gained a number of brief naps—*e.g.*, while undergoing a test of basal metabolism.

The experimenters at George Washington University, whose study is reported (1) by the university as such, without allocation of responsibility for authorship, employed four men and four women as reactors. The ages of the reactors ranged between 17 and 33.

Their formal tests included: (1) five forms of the army alpha; (2) five lists of words the opposites of which were to be written within a prescribed time; (3) the checking of digits as they occurred in "random" tables; (4) the naming of colors; (5) recall of legends on automobile registration tags exposed for two seconds; (6) thresholds of auditory intensity; and (7) extent of peripheral fields of vision.

When one peruses this formidable array of "tests," one is tempted to ask upon what principle they were chosen rather than others. To be sure, the substitution test of Laslett resembles one which previously had exhibited a large difference in performance before and during the later stages of asphyxiation; and which also exhibited a very slight difference in speed—amounting to about one letter in two minutes—before and after ingestion of 60 c.c. of alcohol in a liter or so of solvent—an effect which certainly is not striking. As to the rest, there is not one which had already been proved to have any considerable diagnostic value with respect to moderate doses of any depressant. What conclusion could we draw from a differential effect if one had been found? It happens that the question is academic, for, in no case did a differential effect appear.

Two alternative interpretations arise. (1) No impairment of the reactive apparatus resulted from the insomnia. (2) The tests were not crucial.

The second interpretation is the more conservative. If Hammond is correct in placing a limit of ten days to toleration of total lack of sleep, then, on the first assumption, we have a picture of no damage resulting to the subject from a fourth to a half of the deprivation which would be required to kill him. We do not argue that half this amount should have half-killed him; but we should marvel to be assured that no impairment resulted which *some* detector could record. Perhaps we have given Hammond's dictum too much weight. One of the present reviewers (S) has spent over a year in abstracting and compiling the reports of fact and of opinion of medical authors on the subject of sleep, and is in a position to show that almost every important statement of fact or opinion made by one author or group of authors is contradicted by some other individual or group of at least comparable authority.

However, there are grounds on which the second interpretation may be preferred to the first; we shall proceed to give them.

(1) It more nearly harmonizes with the view of at least one author (21).

(2) In order that score (X) in a test can be evaluated as a measure of "impairment," "fatigue," "intoxication," "intelligence" (any one of which we may designate by the symbol (Y), using a subscript if necessary) it is necessary that we know what "Y" is. That is to say, we must not merely be able to pronounce its name; we must be able to give its dimensions and indicate some unit of measurement. Secondly, we must make *simultaneous* measurements on X and Y, fit them to a curve, and derive its equation. Thereafter, if we have given a value of X we can deduce the value of Y from the equation of the curve, subject to a probable error which can be computed. (The regression-equation obtained through the medium of correlation, is a special example of this method, legitimate when certain definite conditions are met; illegitimate otherwise.) Obviously, if Y cannot be measured directly, we can never know how closely we can measure it indirectly, by measuring X instead.[3] It is idle to speculate on the matter. One had better conserve one's breath for "hallooing and singing of anthems."

[3] The remarks of Muscio (17) on this point would be called commonplace if addressed to a group of physical scientists. They are wasted upon the typical "tester"—not because they are too obvious but because they are too profound.

The usual procedure employed by the "mental tester" is to conceal this rather obvious fact by an argument something like this. This is an impairment-test.[4] That it measures *impairment*[4] is implied in its very name. Therefore impairment,[4] even though we do not know what it is, is what this test shows.

This device is simply an instance of the logical fallacy of equivocation, the "impairment"[4] indicated by the score, X, being falsely identified with some function of what the measure Y would yield and which also happens to bear the name *impairment*.[4]

In the locality in which one of the reviewers (J) grew up, many friendships were ruptured, and some noses broken, in debates over whether John the Baptist was a *Baptist*.

We shall now point out a similar folly: namely, that of arguing that an "intoxication-test" is necessarily a measure of *intoxication*. The same reasoning may be applied to "tests" in general.

In the past, reaction-time has been measured, subject to experimental control of some group of variables; and it has been almost universally argued that the more the variables increased the reaction-time, the more deleterious they were. In recent papers, one of the present writers exposed the fallacy and showed under what conditions a constant variable might increase, and under what conditions diminish, the reaction-time.

The work from which the following example is taken is still unpublished. We prepared a number of lists of numbers, some of two digits, greater than 15; some of three digits. Some of these lists contained the same numbers as appeared in other lists but in a different arrangement; and the lists were so distributed that the reactor had to perform, in the long run, the same tasks, before and after the introduction of an experimental variable. The variable chosen was ethyl alcohol, 30 c.c. in quantity, to which water and suitable flavoring matter were added so as to make the alcoholic concentration 20 per cent. Four tasks were given the reactor, to be performed before, between, and 45 and 75 minutes after, ingestion. The numbers were pronounced by the experimenter; the subject required to respond, as quickly as possible, in test 1 by naming the double of the number; in test 2, its treble; in test 3, by giving the result of adding a given constant to it; in test 4, by giving the result of subtracting a given constant from it. A Dunlap chronoscope

[4] For "impairment" substitute "intoxication," "mental alertness," "fatigue," "intelligence," etc., *ad libitum*.

was employed, with vocal keys, so that it was started by the pronunciation of the stimulus-number, and stopped by the subject's response. The time was measured and averaged; the variability and accuracy were computed. The results of ingesting the alcohol, for one subject, were as follows: For *doubling,* the average time was increased by 0.23″, or 16 per cent, the difference being four times its probable error. The variability, as measured by the standard deviation, was increased by 0.13″, or 29 per cent, which is over three times its probable error. Accuracy, however, was increased by 3 per cent.

For *trebling,* the time was increased by 0.20″; but the difference is only 1.4 times its probable error. The variability was increased by 0.27″, or 26 per cent, the difference being three times its probable error. Accuracy was increased by 3 per cent.

For *adding,* the time was decreased by 0.04″, or 2 per cent; but the difference is less than its probable error. The variability however was decreased by 0.49″, or by 56 per cent; and the difference is seven times its probable error. Accuracy was increased by 2 per cent.

For *subtraction,* the time was decreased by 0.11″, or 5 per cent, but the difference is only 1.3 times its probable error. The variability was decreased by 0.24″, or 26 per cent, the difference being four times its probable error. Accuracy was increased by 4 per cent.

By some "criteria," "performance" was impaired; by others, enhanced. By "clinical standards," the subject, at every sitting, would have been called moderately drunk. We leave the interpretation of the results to those who feel competent to make it.

(3) Our third ground of preference for the more conservative interpretation of these results of insomnia is, that in three of the experiments, behavior was noted, *between the periods of testing,* which suggests that the reactors were really in a bad way.

During the second night, Kleitman (12) reports that one of his subjects "found it impossible to read or to study." On the day following he had great difficulty in taking lecture notes. "After a few words had been written in the correct fashion, his hand would begin to slip, . . . would slide across the paper, and instead of words there was unintelligible scribbling." After the third night, "taking lecture notes was entirely impossible, as the pencil would fall out of the subject's hands after he had been sitting for a short while. . . . Attempts to count one's own pulse were almost always unsuccessful, because the subject would lose trend of the count after he had reached twenty or thereabouts, or else would become extremely drowsy." Laboratory work could be carried on,

possibly, because the external situation was more subject to change. In the George Washington University experiment, one reactor, required to drive a car, soon landed it in a ditch on the *left* side of the road. A member of that group would occasionally attempt to lead the rest in singing, but after a stanza or so would drop into a different song. Kleitman, and also Robinson and Herrmann, report that some of their subjects became very irritable. Kleitman's subjects sought to evade the rules of the experiment and steal the opportunity for sleep, resorting to deception in the attempt. Kleitman himself, while engaged in looking up logarithms for an experimenter, responded correctly; but once, instead of reporting the logarithm required, declared, "It is because they are against the system." Being asked to explain, he replied "that all the time he was looking up the logarithms he had been under the impression that he was having a heated argument with the experimenter on the subject of labor unions."

We suspect that if disturbances of behavior such as these had followed upon the ingestion of alcohol, instead of insomnia, the reactor would be called drunk; if their cause could not be found, and if they occurred often, he would be called insane. If such aberrations happen to occur in critical situations, disaster may result from them.

The reviewers take this occasion to remark that delirium not infrequently results from loss of sleep, far less extensive than occurred in these instances. Among the various conditions which favor it are repetitive activity, such as operating an automatic machine, plowing, or cross-country driving. In these experiments the reactors were very frequently subjected to social stimulation; so that such contributing factors as we have mentioned had not the best chance to operate.

Deprivation of sleep has, in fact, been systematically employed for centuries, in religious orders, as a means of inducing a state of delirium; the latter being taken as a reward for the effort. The medicine-man uses Bhang, or takes hasheesh, or chews hemp-leves, or the mescal button, to attain delirium by means of narcosis. The prophet fasts "forty days and forty nights," or "for three full weeks," and attains it by starvation. The dancing dervish, like some members of the "schools of the prophets," may "dance before the Lord, and dance with all his might," and attain it through physical exhaustion. The hermit and the monk keep vigils—spending the

night in pacing back and forth as they repeat their devotions; and not infrequently the expected visions occur.[5]

The result of these various methods, in the light of modern theories of cellular physiology, is essentially the same. The behavior of the individual as an organism is very similar in the several instances, in that behavior is no longer determined, in high degree, by the action of external stimuli; but rather, by repetitive, perhaps circular, activity of reaction-arcs whose receptors are internal to the body. In a sense, he is actually "freed from the domination of the external world," as the ecclesiastical writers assert.

To be sure, the quality of the "revelations" thus obtained is not uniformly high. In primitive communities, if a prophet shocked the members without convincing them, they were likely to stone him to death as a blasphemer; and such records as we have, suggest that at least at times, the mortality rate among the Jewish prophets has been rather high. In highly organized institutions, such as the Catholic Church, the superiors pass judgment on the members' reports of revelations. That judgment is usually very conservative. The piety of the individual who seeks them is approved unless he seems unduly ambitious; but few of the results of the search have ever received the stamp of authority.

There is an old saying, "*In vino veritas.*" Its basis is probably sound. The reaction-system of an individual tends to deteriorate in narcosis; the more stable patterns tending to persist the longer.

[5] The present writers are indebted to a colleague for the following additional suggestion: The fact of disorganization of behavior under conditions of fatigue and insomnia is utilized in the practice of the "third degree" examinations in American police stations. The patient is kept awake, and is questioned rather continuously, through a comparatively long period, the theory being that his habits of bedaving consistently with a manufactured story may break down earlier than his more stable habits.

To this suggestion the writers would add, that the testimony induced by this means should not be considered reliable, unless it be strongly supported by other evidence; for the procedure employed is essentially a form of torture, no less cruel than much older forms, if it be continued long enough. Just as the victims of the rack would make false confessions in order to end the torture, even though confession meant death by hanging or burning: so, it may be expected that after two or three days of deprivation of sleep and continuous questioning in the "third degree," the suspected criminal might testify to anything in the hope of being let alone. A colleague, who has undergone a number of periods of experimental insomnia, testifies that the discomfort he suffered is almost indescribable; and that his dread of future experiments exceeds his dread of any other form of "physical" pain.

Hence when deterioration is but partial, the patterns which are most conspicuously exhibited are the stable ones, whose organization constitutes the "true character." For similar reasons, one may say, "*In fatigatione veritas,*" since exhaustion, whether induced by excessive effort for a short time, or by ordinary effort continued for a long time without opportunity to rest, resembles narcosis in all its more prominent phases.

We have stressed this interpretation of these experimental results because of the widespread tendency among the popular and pouplar-scientific writers to give them a superficial interpretation: namely, in the words of Kleitman (12), that the "mental power of the subjects" remained normal despite the disturbances of behavior which were informally observed. And of the reporter of the George Washington University (1): "The mental alertness of the subjects seemed little affected by the prolonged period of insomnia. In fact, they did just as well or better on the mental tests at the end of sixty hours of sleeplessness as they did at the beginning of the period."

In the study of Lee and Kleitman (12a), one subject, a healthy male student, 28 years old, was required to undergo seven periods of deprivation of sleep; three of which lasted 60 hours each, two 90 hours each, and two 114 hours each. The experimental periods were widely separated in time, and were distributed over almost a year.

The authors, noting the seeming discrepancy between the subject's description of his own condition and the diagnostic failure of the tests employed by Kleitman and by Robinson and Herrmann, decided to extend the search for indications of disorganization of behavior which might lend themselves to quantitative expression.

They "wished to include only those (tests) which in the hands of other workers had been found capable of demonstrating the effect of fatigue and of drugs, and which were found suitable for long repeated use and quantitative treatment." The tests they selected fall into two classes: those in which, in the opinion of the authors, "effort" is not directly a factor; and those in which it may be. We shall enumerate the tests and summarize the results together.

Group 1: (a) *Knee-jerk.* At each sitting the subject was required to rest for ten minutes in a reclining chair, after which the patellar tendon was struck ten times, at intervals varying between 15 and 30 seconds, by means of a spring-driven hammer automatically released. The extent of the movement of the foot was registered on moving paper. The knee-jerk was not affected by insomnia, but vanished

when the subject fell asleep, as he sometimes did during the experiment.

(b) *Pupillary reflex.* The diameter of the pupil in dim light was measured under normal conditions and under insomnia. Under the latter condition its normal diameter is diminished by about 8 per cent. The contraction from stimulation by a bright field was also measured under the two conditions. In insomnia the change is diminished by about 25 per cent. The measures of variability from the averages are not given.

(c) *Threshold for stimulation by induced electrical current.* Martin's method was employed. No differential effect was observed.

(d) *Steadiness in standing.* The subject was required to stand still for two minutes with the eyes closed. A recording device fastened to the head traced a record on a paper fixed above it. A very marked increase in unsteadiness appeared during insomnia.

Group 2: (a) *Reaction-time.* At each sitting 40 simple reactions were made to an auditory stimulus, which was presented 1 to 10 sec. after a warning-signal. Also, 20 reactions were required to be made to either of two colored lights indifferently; interspersed among the 20 "appropriate" stimuli were 12 presentations of other lights, to which reaction was to be inhibited. No differential effect appeared during insomnia.

(b) *Naming of opposites.* Fifty cards were handed the subject, each of which bore a word whose opposite he was to pronounce as quickly as possible. The time required for giving the whole list was measured. No effect of insomnia was detected.

(c) *Naming of colors.* Hollingworth's method was employed. An array of only 100 colors was named with normal speed and accuracy during insomnia; but an array of 1,200 colors, which required a longer duration of sustained effort, required more time under insomnia than normally; the accuracy being also reduced. The authors give no measures of *normal* variability.

(d) A cancellation-test, adapted from the one used by Franz, and (e) speed and accuracy in "mental" multiplication, were also employed, but showed no effect which was ascribable to insomnia.

Physiological Changes Associated with Sleep. Tuttle (25) set out to study the variations in the patellar tendon reflex produced by various agencies. He employed an apparatus which subjected the tendon to blows of constant strength, repeated at the uniform rate of eight a minute. The patient sat in a barber's chair during the experiment, which, in some cases, lasted for an hour. On three occasions the patient fell "asleep." The reflex disappeared during such times,

reappearing at such times as the patient stirred. The finding tends to confirm those of Lombard and of Lee and Kleitman, whose works the author cites.

Landis,(14) using Erlanger's method of obtaining graphic registration of blood pressure, found that when the patient fell asleep, both systolic and diastolic blood pressure dropped by about 26 mm. Hg. A pressure rhythm, more definite than during the waking period, was evident during sleep. Sudden awakening produced a sudden rise, while gradual awakening produced a gradual rise. Bodily position was without influence upon the record. Landis suggests that the circulatory changes may be resultant from sleep, rather than causes of it. Since blood pressure is dependent in large degree upon the tension in the system of skeletal muscles; and since a rather general state of relaxation seems to characterize what is usually called "sleep," the argument sounds reasonable to the reviewers.

Kleitman (13) found that absolutely more phosphorus is excreted in urine during sleep than during wakeful activity. Long continued medication with potassium bromide, which acted usually to keep the patient "sleepy" (though occasionally irritable), had no effect. The author thinks it probable that the excretion of phosphorus is "independent of intellectual activity." Excretion of phosphorus does not parallel excretion of acids. Increased muscular activity was followed by increased excretion of phosphorus; but the effect was not found until the patient had entered upon a period of rest. The facts suggest that elimination of certain waste products can be best carried on while the cell is in a "resting" state.

Kleitman's study (12) of the effects of insomnia included measurement of the following: content of blood-sugar; alkaline reserve of blood and plasma, derived from measurement of content of carbon dioxide; percentage of hemoglobin; numbers of red corpuscles and of leucocytes; concentration of blood cells; body weight; rate of consumption of oxygen; and mean temperature. None showed any consistent or important variations from the normal as a result of prolonged insomnia. The body temperature, which normally undergoes a cyclic variation from hour to hour during the day, exhibits a decreasing range of variation with increased loss of sleep. Excretion of nitrogen and of creatinine shows little diurnal variation, and is not affected by insomnia. These findings are a little out of line with the expectations aroused by the studies of de Manacéïne and of Crile and others; but there is no apparent reason for questioning them.

The experimenters at the George Washington University (1) found a decrease in hemoglobin and number of red cells, and an increase in the number of leucocytes, resulting from experimentally induced insomnia. Acetone appeared in the urine after 60 hours' loss of sleep. No change was found in blood-sugar or in the rate of oxygen consumption; and but little in lung capacity. One subject showed a slight increase in the quantity of indican excreted. The report asserts that the tests showed a slight loss of physical strength; but does not indicate how much.

Reed and Kleitman (19) made a careful study of respiration during "sleep," including 30 experiments on nine subjects. As we mentioned above, they found the inversion of the ratio of the extent of thoracic movement to extent of abdominal movement did not invariably occur when the subject fell asleep; in fact, they observed it in only half their experiments. In some cases a periodic irregularity resembling Cheyne-Stokes breathing appeared, which sometimes characterize only thoracic and sometimes only abdominal respiration. In general they found "such a variety of changes concomitant with wakefulness, somnolence, or sleep, as to conclude that there is no direct relationship between these varied states and the rate and regularity of respiration." Respiration, they argue, is very easily affected by the changes in pattern of nervous flux; much, if not most, of this flux, in wakefulness, is generated in intramuscular receptors, and is probably greatly diminished in sleep, as a consequence of muscular relaxation. Hence, respiration in sleep is usually more regular; but, they conclude, "The classical notion of very marked and constant changes in respiration symptomatic of sleep should . . . be abandoned." They found that auditory stimuli too feeble or too infrequently repeated to elicit the specific response of pressing a reaction key often produced a non-specific modification of respiration.

Aron (2) compares the rates of basal metabolism during sleep and wakeful activity in the case of two adolescents.

Theories of Sleep. Haberman (5) reviews the various types of theories now classical, and, like Herrick (6) concludes that none are adequate. He proposes a theory based on the assumption of "de-energization" of nerve cells, probably ganglionic; the state being accomplished either through waste products, diminished stimulation, suggestion, or habitual "psychophysical sequence." The diminution of activity need not be general in order to produce a state which he would call sleep; there may result, through suggestion, or habitual

sequence of reflexes, a dissociation of subsidiary systems of arcs from the whole. In pathological "sleep" deleterious products dysenergize, rather than deënergize, the ganglionic cells. The reviewers do not fully comprehend the distinction intended to be made.

Johnson (9) assigns the word "sleep" to denote a condition characterized by (1) a high degree of relaxation in the general system of postural musculature, accompanied by (2) an enormous increase in the magnitudes of the stimuli capable of arousing a specific response. Since it may be supposed that the greater portion of the nervous current utilized in bringing about coördinated activity is generated in intramuscular receptors, he proposes that its absence, however produced, be regarded as the cause of the inactivity found in sleep. That is, any agent capable of producing sufficient muscular relaxation will produce the kind of behavior he calls sleep. Fatigue toxins may be one agency; but if they systematically accompany other stimuli, the latter, by the laws of habit, may become effective substitutes for them, and may later serve regularly to induce "sleep" in the absence of a "bodily need." Certain resemblances between this hypothesis and those of Haberman and of Coriat are rather obvious; but in the form proposed a more definite mechanism is assumed; and a more definite problem set for experimental attack. On such an assumption, the "depth of sleep" could be measured in terms of the tension of postural musculature. The hypothesis may require amendment to provide for alternation of activity and rest among certain subsidiary systems of musculature; and for the possibility that relaxation may be a manifestation of a positive form of activity in special types of muscle cell, requiring continuous, or frequently repeated innervation, to maintain it.

Kleitman (12) by implication, almost suggests such an explanation; but explicitly reverts to an assumption of "multiple levels" of activity in the central nervous system. It is doubtful whether such an assumption does more than rename the observable fact that activity in sleep is diminished, not merely in quantity, but also in respect to integration. The allocation of "centers" and "levels" in the central nervous system has never been an easy problem; and the experimental work of Head, Franz, and Lashley have raised a very strong presumption that such allocation is not, indeed, a fact.

Miscellaneous. Fleming (4) presents the results of a questionnaire sent to college trained mothers, and intended to cover the sleeping habits of 120 children. Complete returns were obtained on

78 children. The data are such as are usually yielded by the method employed to obtain them.

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HYPNOTISM

BY PAUL CAMPBELL YOUNG
Louisiana State University

Since the phenomena of hypnotism are even to-day in great doubt, the major part of this paper will be taken up with a critical consideration of the major characteristics of hypnosis as it is popularly described. Theories, uses, hypnotizability, and so-called dangers of hypnotism will be more sketchily treated at the end of the article.

Phenomena of Hypnotism

At the very beginning, it is well to distinguish between two classes of hypnotic phenomena: (a) those that appear spontaneously, and (b) those that, though not arising spontaneously, *can be* evoked in hypnosis. For the proper evaluation of theories of hypnotism this distinction seems a prerequisite. If, for example, exclusive *rapport* occurs automatically upon the induction of the deepest stage of hypnosis, a theory of hypnosis based upon the fact of *rapport* would have a validity quite different from the same theory if *rapport* does not necessarily appear in any stage of hypnosis.

Phenomena Spontaneously Arising in Hypnosis. According to the ideas of pre-scientific animal magnetism, which still holds sway in some quarters, the mere induction of trance in a subject gave him marvelous, or at least inexplicable, powers. Clairvoyance,(1) telepathy,(1) transposition of senses, action without suggestion of drugs securely enclosed in glass,(2) states in which "The ordinary obstacles of space and time vanished to her"(3)—some or all of these ensued upon the subject's going into hypnosis. This conception of hypnotic conditions still persists in Alrutz's claims that upward passes spontaneously bring about hyperaesthesia, whereas downward passes tend to bring about anaesthesia in the hypnotized subject, even though the subject is unaware of the passes, from which, also, the subject may be shielded by thick glass or other materials, and even though the operator does not know what effects to look for.(4) The following from Boirac is typical of the neomesmerists: "Not only can magnetic action produce its effects independent of any 'suggestion' influence, but it also can, in certain cases, prevent and

annul the effects of suggestion."(5) Boirac also speaks of "the psychic force of the operator emanating from every portion of his body."(6) Kindborg considers this mysterious influence physical rather than psychic, as shown by his calling it "the physical effect of a force as yet unknown to us, as Mesmer in the eighteenth century thought." Kindborg, who says that "Alrutz, like me, assumes a radiation effect," claims that the use of magnets in hypnosis excites phenomena inexplicable on the theory of suggestion.(7)

One answer to such claims as those just referred to is that, since such phenomena do not appear spontaneously in the experiments of other operators, they are not essential concomitants of hypnosis, even if it could be proved that the conditions under which they are elicited are strictly scientific. But Levy-Suhl calls attention to the past failures of mesmerism to withstand scientific inquiry and asserts that Kindborg "fails to recognize certain suggestions exercised by him, and this leads him to the assumption of unknown forces." (8) The writer would cite the following as an example of Kindborg's scientific naïveté: "I cannot see any suggestive influence in the simple laying of the hands on the head. In spite of that, several hypnoses, unimpeachable according to our opinion, were obtained under these circumstances."(7) No suggestive influence when Dr. Kindborg seeks to cure his patient by laying hands on his head, in a hospital!

Having mentioned the preceding matters merely for their historical interest, we come to problems which are by no means settled. One of these is, do catalepsy, post-hypnotic amnesia, and *rapport* spontaneously arise in artificial somnambulism, and are they a kind of criteria of hypnosis? For obvious reasons this question cannot be settled by an appeal to books on hypnotism. Any one who reads the literature of hypnotism is struck with the disagreement among writers, not only on the essentiality of the three characteristics just referred to, but also on others equally as vital. To decide which authorities should have precedence over others is a bootless task, and one which will not be attempted here. There is a simpler way out of the difficulty: it matters not how many times as reported by trustworthy operators these phenomena above mentioned do occur in hypnosis, if they do not appear spontaneously in the cases studied by equally responsible men, they are not essential characteristics of hypnosis as such. In the latter case, they should be put in the category of phenomena that *can be evoked* in hypnosis rather than in that of phenomena which inhere in hypnosis. There is all the

more reason for this rule inasmuch as most people who are hypnotized have not only heard either from laymen or hypnotists that catalepsy, *rapport*, and post-hypnotic amnesia would appear, but also have seen subjects who exhibited all of these phenomena.

Catalepsy. Though catalepsy is easily induced in hypnosis, according to Moll it is not an essential condition of any stage of hypnosis. He says, "I have hardly every clearly seen a typical *flexibilitas cerea* in hypnosis, except when the training of the subject had been directed to that point."(9) Bramwell's words are to the same effect: "Further, once the eyes have been closed by suggestion, rigidity of the limbs does not follow as a natural sequence, but requires to be suggested in its turn. Thus, if during hypnosis I lift the arm of an untrained subject, it will fall directly I release it."(10) If an experimenter begins with passes, then proceeds to bend the subject's arm, keeping the elbow on the couch and holding the hand in the air ever so short a time, the subject gets the suggestion that the thing to do is to leave the hand where it has been placed. From this simple posture to one in which the leg, when raised, keeps the position without falling back again to the couch is only a matter of further, unexpressed, but still very easily felt, suggestion. By avoiding manipulating the subject's limbs, the present writer found that none of his somnambulistic subjects ever showed any tendency toward spontaneous catalepsy.

Post-Hypnotic Amnesia. That the memories of the subject for the events of a given hypnosis are affected to some extent with most methods of inducing hypnosis is a commonplace observation. That total, or even considerable, post-hypnotic amnesia, however, is a criterion of even the deepest stage of hypnosis is very questionable. It is pointed out by the writer, of course, that the presence of considerable post-hypnotic amnesia along with other characteristics, *e.g.*, suggested analgesia in hypnosis, shows somnambulism to have taken place. The question at issue, though, is whether remembering all or most of the events of the hypnotic session thereby proves the subject not to have been somnambulistic.

Since the great majority of past writers considered post-hypnotic amnesia as a *sine qua non* of somnambulism, there has grown up a kind of post-hypnotic amnesia-tradition, which, being circulated by those who lecture and demonstrate, is then confirmed, because these same lecturer-demonstrators (strange to say!) find it exemplified in their own subjects. Thus Hadfield's statement, "Amnesia is perhaps the best single test of hypnosis as distinct from the hypnoidal con-

dition,"(11) and Mitchell's, "In the hypnotic state there is recollection, actual and potential, of all the events of waking life, whilst in the waking state there is no recollection of the hypnotic phase,"(12) are typical of the way in which the ideas of earlier authorities have been copied down and perpetuated by later ones. So, too, does Wells express the usual conception in speaking of "the long and practically universal recognition given to amnesia (following the state) as a test of hypnosis."(13)

This amnesia tradition, though, has been denied wholly or in part by Bernheim,(14) Moll,(14) Heidenhain,(14) and Löwenfeld.(15) Even Bramwell says "in other instances it is absent in the deeper ones [states of hypnosis] characterized by alterations in the special senses."(16)

There are several reasons for doubting that spontaneous post-hypnotic amnesia is a *sine qua non* of any stage of hypnosis:

1. It is almost or quite universally agreed that a suggestion given in hypnosis to remember the events that are then passing enables the subject on awaking to recall substantially all that happened. If amnesia were an essential characteristic of somnambulism, it would occur as readily after the suggestion (given in hypnosis) to remember as after the suggestion to forget. However, the use of hypnosis as a means of resynthesizing the personality (17) depends on the fact that post-hypnotic amnesia does not always exist.

2. In characterizing the so-called post-hypnotic amnesia there has been a confusion of inability to recall with inability to remember. Most subjects who are unable to recollect the incidents of the séance easily recognize them when they are mentioned.

3. Experiments (18) show that some subjects can make up their minds, before going to sleep, to do all that the operator demands in hypnosis, but to remember upon awaking all that happens in hypnosis; and thereupon can exhibit almost perfect recollection of the hypnotic events, among which may have been the whole *ensemble* of somnambulistic characteristics. This may occur even though the operator had explicitly ordered them in hypnosis to forget all that happened.

4. Experiments show that when subjects are taught definite associations in hypnosis, there is never total loss of the association on awaking. Although the subject may not recall having learned the material, there is a saving in the relearning.(19) The greater the number of associations of the same general kind that have been learned in waking and in hypnosis, the less the distinction between

the two sets of memories. A given hypnosis is no more invalidated by post-hypnotic memory for what took place in it than a given period of sleep would be by the remembrance on awaking of what one had just dreamed.

It seems probable, then, that post-hypnotic amnesia, which is never complete, should be classed with the phenomena that *can be* evoked in hypnosis, and not with those that are essential to it. Whether there is amnesia, and if so how much, depends on the method of hypnotizing, the suggestions that are given in hypnosis, and perhaps above all, on the autosuggestions of the subject, or on his expectations.

Rapport. Catalepsy and post-hypnotic amnesia, though usually considered essential hypnotic characteristics, are nothing like so important theoretically as *rapport*. McDougall says "*rapport* is of the essence of suggestion."(20) He does not, however, consider it as always exclusive *rapport*.(21) Coriat says: "Thus the four most important symptoms are, loss of initiative, loss of memory, increased suggestibility, and the *rapport*, or state of dependence, between the subject and operator."(22)

Since the question of *rapport* is so important in forming a theory of hypnosis, it may be well to ascertain what some late writers understand by the term. Friedrichs says *rapport* (Bindung) is analogous to the affective connection between individuals in general (Liebesbindung).(23) Straus considers it a connection of soul and soul, and an interplay of mental contents such as contradicts the individualistic theories of hypnosis. Normally, a communication is experienced as coming from a personality, but in hypnosis the communicated content and the communicating personality are experienced as one. The acceptance or rejection of a suggestion depends not only on the "meaning" of the suggestion, but also upon the relation of subject and operator. The opposite of *rapport* is seen in daily life where we repudiate everything that comes from a personality that we repudiate or hate; because the personality is repudiated we reject all that comes from him.(23) Haupt, however, finds that the hypnotized subject ordinarily "does not think of the person of the questioner [the operator] but only when he is asked with whom he is speaking." (24) The Freudians think *rapport* is a kind of transference. Jones states that in Freud's view the hypnotist takes the place of the ego-ideal, *i.e.*, the thought of him becomes identified in the unconscious with that of the father.(25) McDougall speaks of "The *rapport* established between *A* and *B* when *A* has

hypnotized *B*, the moral relation of ascendency, of prestige," (26) . . . "*A* exerting upon *B* a moral influence that inclines *B* to accept with conviction whatever proposition comes from *A*." (27) This is essentially Binet's idea also.(28) Bleuler holds almost the same opinion.(29)

The old mesmeric conception of the relation of hypnotist and subject was that of sender and receiver of magnetic influence; the conception of McDougall and Bleuler is that of sender and receiver of moral influence. Mesmer postulated the setting up of magnetic currents; McDougall postulates the setting off of a powerful instinct. Most other late writers speak of *rapport* as essential to hypnosis, and in doing so they are perpetuating the traditions of the Nancy school. Bernheim, for example, claimed that *rapport* is the sole difference between hypnotism and sleep.(30)

The considerations which make doubtful the essentiality of *rapport* in somnambulism are varied:

1. The findings of previous experimentors. In spite of the writer's purpose not to pit authority against authority, it should be said that among the many who repudiate the tradition by considering *rapport* as an artifact are Braid,(31) Moll,(32) and Bramwell; all of whom consider *rapport* the result of direct and indirect suggestions made by the operator or by self-suggestions which arise from the subject's conception of the nature of the hypnotic state. Thus Bramwell says, "The *untrained* somnambule responds with equal readiness to the voice of anyone, and, if he has been taught only to respond to one voice, he still hears others." (33)

2. In the mediumistic trance and kindred states there is no *rapport*. The "control," which really takes the place of the hypnotist, so far from being another person, is simply another part, or another state, of the medium's own mind. So, too, when sitters at table-tipping go into trance, they are not in *rapport* with any one person.

3. Religious ecstasies present a clear case of somnambulistic states, analogous to hypnosis, without *rapport*. Hallucinations, illusions, analgesia, varying degrees of amnesia, all follow intense concentration of the mind on certain ideas, quite apart from personal influence.

4. Quiescent states of self-hypnosis as practiced by the Hindoos and others are attained by contemplation and self-stimulation, without requiring *rapport*.(34)

5. Autosuggestion as expounded by Coué (35) and Baudouin (36) has made definite a technique of hypnotizing in which

the operator has only a minor rôle, if any at all. Self-hypnosis has been known for a long time, at least since the days of Braid (37) and Baragnon (38); but not until recently has it been thought possible by reading a book to give oneself suggestions which are carried out with as much ease as suggestions are carried out in hypnosis. It is interesting to see how those who believe in the primacy of heterosuggestion try to explain away the facts of autosuggestion. For example, McDougall says, "All so-called autosuggestion is, in reality, heterosuggestion" (40) because "*suggestion* essentially implies the influence of one person upon another." Autosuggestion "is, in reality, suggestion from the operator, renewed in his absence by the repetition of his words accompanied by the imagination of his personality." (41) For McDougall *autosuggestion* is reserved for cases where there is a division of the personality, and where it would seem that one of the two partial personalities may exercise suggestion upon the other.(41) McDougall would make autosuggestibility, as Charcot made hypnotizability, depend upon the possession of hysterical traits. McDougall's ideas would have more force if everybody had to be hypnotized by another before being able to hypnotize himself. Even then, however, as Bramwell points out, the fact that autosuggestion is learned through heterosuggestion no more makes the former depend upon the latter than learning to swim with a life-belt makes winning a swimming championship depend on using a life-belt.(42)

The psychoanalytic conception is more helpful than McDougall's in understanding the relation of suggestion and autosuggestion. As expressed by Jones, it is as follows: "We can no longer regard the subject as a helpless automaton in the hands of a strong-willed operator. It is nearer the truth to regard the operator as allowing himself to play a part, and by no means an indispensable one, in a drama constructed and acted in the depths of the subject's mind." (43) Schilder holds likewise.(44) "It is extraordinarily difficult," says Jones, "to draw any sharp line between hetero- and autosuggestion. The relationship is so very intimate as to make it probable that the agents operating in the two cases are merely variants and not distinct forces." (45) "The one point in which the two conditions differ is in respect of the ideas on which concentration has taken place. With heterosuggestion we know that this is the idea of the Father imago, which has been aroused through a suitable substitute. With autosuggestion all the evidence points to the idea being that of the actual self." (46) "The essential

agent in both is narcissism. Since primary narcissism is more fundamental than the Father ideal itself," (47) Jones tends to agree with Baudouin that autosuggestion is more fundamental than heterosuggestion. Still, perhaps, "the latter process may prove in most cases in practice a necessary stage in the evocation of the former."(47) There is another simple way of comparing the relative importance of suggestion and autosuggestion. Namely, the result when there is a conflict between the two in the hypnotic state. This point is important enough to be taken up separately.

6. Failure of *rapport* in the natural course of hypnosis. Those who contend most earnestly for the importance of *rapport* for a theory of hypnotism still admit the limits of *rapport*. For example, it is agreed that the subject will not obey the operator in committing an act which is repugnant to the former's moral scruples.(48) Many have found that hypnosis cannot be induced in case the subject does not desire it.(49) As McDougall says, "I say to the subject: 'On this occasion do exactly as usual, with this difference only—make up your mind that you will remain wide awake in spite of all that I say to you.' I then find it impossible to induce hypnosis." (50) Besides, there are many clear cases in which the operator's suggestions were disobeyed for no special reason. Prince,(51) Bramwell,(52) and McDougall (53) give cases understandable only on the supposition of all-powerful autosuggestions; as does Serog, who says that the deeper the hypnotic state the greater the autosuggestibility becomes.(54) It is here that one should classify Haupt's Stirn- und Nachenhand method, by which, according to Haupt, if the "forehead and neck-hand remain lying on the head of the subject when the command to wake up is given, he is every time unable to awake." (55) "Suggestions which were opposed to the effects of the Stirn- und Nachenhand were were not received by the subjects." (56) Strangely enough, Haupt thinks that the precise location of the hands is important. What Haupt has obviously done is to make so prominent the placing of the hands on the subject that, so to speak, the subject is in *rapport* with the hands in spite of the operator's "moral influence." This physical *rapport* is the only kind of *rapport* which the mesmerists ever evoked.(57)

7. Definite experiments show that *rapport* is mainly, if not entirely, an artifact of the method of hypnotizing and of the autosuggestive attitude of the subject. In a series of experiments at the Louisiana State University conducted in May, 1926, by Professor I. P. Foote and the writer, with three somnambulistic students who

had been selected from among twelve volunteers (because of the depth and certainty of their hypnotic states) the relation of *rapport* to hypnosis was studied. After all three subjects had been hypnotized separately on two different days, and after it had been determined that the writer could induce in them all the classical concomitants of hypnosis: catalepsy, positive and negative hallucinations, gross illusions, analgesia on puncturing the skin with a pin, hypnotic and (so-called) post-hypnotic amnesia, etc., they were directed in waking by Professor Foote to give themselves autosuggestions as to what they could and could not do in a state of hypnosis induced by the writer. Unknown to the writer, each man would, before coming to a séance, sign and place in his pocket some such statement as the following: "While in hypnosis, I will obey all commands except that of becoming insensitive to pain"; "I will submit to hypnosis at the hands of Dr. Young and will carry out all the suggestions until I hear one rap. When I hear two raps I will raise my arm, make three pats with my right foot, and stretch out my left leg. When I hear three taps I will again carry out Dr. Young's orders. When I hear four raps I will wake up." These are respectively the simplest and the most complicated autosuggestions given in the series. One autosuggestion, however, was to the effect that the subject would submit to hypnosis, but when fully hypnotized would not respond to the operator at all, but would respond to the voice of Professor Foote. In this series of experiments three séances were held with one subject and two with the other subjects. The results show that the subjects, by prior autosuggestion, could exhibit whatever degree of *rapport* they decided upon, ranging all the way from being out of *rapport* with the operator on only one suggestion to being out of *rapport* during the whole séance, and responding only to another person's raps. In every case the bona fide nature of the hypnoses is vouched for by the writer's judgment *re* the subject's physical state, by the exhibition of hallucination and analgesia during the séances, and the subject's post-hypnotic reports; and in several hypnoses by other symptoms as well. The operator never knew what autosuggestion the subject had given himself; and upon the subject's beginning any self-instituted movements vainly tried with all the authority of his prestige to counteract them. The subjects reported after hypnosis that while they were out of *rapport* with the operator, he simply dropped out of the picture, and when they came back into *rapport* "there he was, standing by the chair." In the interim the operator had futilely endeavored to evoke responses to

his suggestions. From these results it is likely that any somnambulistic subject could be in *rapport* with any set of stimuli whatever, and that both exclusive *rapport* and lack of *rapport* have the same basis, in the overt or tacit autosuggestions of the subject. This conclusion is more or less in agreement with that of Rosenow, to the effect that the subject's responses to the suggestions of the hypnotizer arise out of the behavior of coöperating.(58) *Rapport* is a matter of coöperating; and it is probable that the amount of *rapport* can be predetermined by the subject's making up his mind before hypnosis as to the amount of coöperation toward the operator, and toward any other sources of stimuli,(59) he will exhibit.

Résumé. If, then, it is doubtful that marvelous magnetic powers flow into the hypnotized subject, or that catalepsy, post-hypnotic amnesia, and *rapport* inhere in the hypnotic state, what are its essential characteristics? According to Coriat (60) loss of initiative and increased suggestibility would be left. If, however, hypnosis can exist without *rapport,* suggestibility as commonly understood may not necessarily be increased. In that case, Baudouin's analysis of suggestibility into acceptance of an idea and the tendency to carry out whatever idea is in mind (only the last of which he really calls suggestibility) may help in understanding hypnosis. In his opinion, "suggestibility" is always increased in hypnosis whereas "acceptivity of another's ideas" may or may not be.(61)

Rivers says that hypnosis is a complex blend of "heightened suggestibility, heightened sensibility, suppression, and dissociation." (62) The question whether heightened sensitivity is aroused spontaneously, may well wait until it is proved that it *can be augmented* by hypnotic command. This question will be taken up next. It may be remarked that Bramwell considers the subject's power over his own body as the essential characteristic of hypnosis.(63)

Phenomena That Can Be Induced in Hypnosis. Aside from the spontaneously appearing states of rest, indifference, passivity, and depersonalization, and the physical state of relaxation, hypnosis is an *ensemble* of characteristics that can be evoked under the hypnotizer's direction. These characteristics are *rapport,* contractures and functional paralyses, amnesias which last during the state or to some extent outlast the state, hallucinations both positive and negative, all kinds of illusions, anesthesia, analgesia, and automatisms. Writers also report control of menses (64) and of lactation,(65) and regulation of childbirth.(66) Blister-raising is extremely doubtful.(67)

Can the physical and mental functions be increased in hypnosis?

Although the current hypnotic tradition is that they can be, the weight of authority is so equally balanced on the question, and the problem of determining differential abilities is so delicate, that it is best to discard all the results of the past unless, along with the results, is published a statement of the methods of investigation.

Comparison of Abilities in the Normal and Hypnotic States.(68) Past experimentation in the field of hypnotism has not been highly successful in establishing the capability-differentia of hypnosis, *i.e.*, what the hypnotized subject *can* do in hypnosis that he *cannot* do in waking. The methods in the past have been of three kinds:

1. Nonscientific investigation, *i.e.*, investigation without definite control conditions. With this method the subjects showed as great an increase in various mental and physical functions as the trickery of the operator and the credulity of the onlookers made possible.

2. Avowedly scientific investigation of hypnosis as an isolated phenomenon, *i.e.*, with control-conditions in hypnosis, but without a comparable investigation of the same persons in ordinary waking consciousness. Under this head would come the extra-laboratory induction of so-called hypersensitivity (69); finding marvelous perceptual acuity, as in the case of Bergson's boy-subject, who read out of the cornea of a man in front of him the three millimeter letters of a book which the man held behind the boy's head, the whole image on the cornea being no more than one millimeter in height.(70) Performance of great feats of memory (71); such carrying out of time-suggestions as, on the face of it, seems far beyond waking ability.(72) All "scientific" investigations of this sort are vitiated by the fact that the normal powers have been grossly underestimated, and that the possibility remains that the very persons who showed such remarkable powers in hypnosis could have done just as well in the normal state if they had been given to understand that fully as much was expected of them. In fact, if one begins to cite wonders because they are wonders, one need not go to hypnosis for even a thousandth of one's cases.(73) Scientifically satisfactory, however, was the method of Luckhardt and Johnston in ascertaining that gastric juices are secreted in hypnosis on the suggestion of a meal (74); also that of some Germans; see a later article.

3. The comparative method, *i.e.*, comparing the performance of subjects while they are hypnotized with the performance of the same subjects while they are in the normal state under exactly similar conditions (except for the matter of hypnosis). Aside from some physiological experiments by certain German scientists, it seems that

this method has been consistently used in only two series of experiments. The first is that of N. C. Nicholson of Johns Hopkins University, in which he found that " The increased efficiency [as shown on the ergograph] during hypnosis shows itself in three ways, (1) By an increase in the actual amount of work done. In fact during the hypnotic sleep the capacity of work seemed endless, as we invariably had to conclude the experiments because of the limited surface of the drum. (2) By an increase in endurance. (3) By a decrease in fatigue, both subjective and objective." (75) The second is that of the present writer, who has reported a series of experiments which tend to show that on the whole there is no noticeable difference between the normal and hypnotic states in the ability of normal persons in the fields of sensation, perception, finer discriminations, present memory (learning and retention), or physical work which does not involve fatigue.(76) There seems to be a great difference, however, in the ability to remember long past events.(77)

All other investigations which were meant to be strictly comparative in method are open to one or all of the following criticisms: (a) Often the comparison has been a desultory, not to say haphazard, matter of a few trials in waking and a few trials in hypnosis, without any standardization of conditions. On the very face of them, most of the reports of comparative studies are worthless as scientific data. (b) There has been doubt as to what state of consciousness was being investigated. Here should be mentioned the work of J. J. B. Morgan and L. E. Travis at the University of Iowa. After having instructed their subjects to day-dream, they found, with ten subjects, that four had lower thresholds, four had higher thresholds, and two had the same thresholds of hearing in reverie as compared with normal consciousness. The experimenters thereupon predicated of hypnosis the results of those subjects who had lower thresholds in reverie. The very point to be proved! Later they found that " Every person whose threshold lowered during the reverie and on whom the attempt was made could be hypnotized, and not a single one whose threshold raised could be hypnotized."(78) (c) The number and order of the experiments, the number and character of the persons acting as subjects, and the exact technique in the hypnotic and waking sessions have not been fully published along with the results—or else when so published, the conditions vary in other respects besides hypnosis or waking. This criticism holds for the work of N. Ach,(79) E. Trömner,(80) A. Chojecki,(81) and to an extent of all previous experimenters.

Thus, Morgan's and Travis' instructions in the two states are different enough to explain any diversities in their results. So, too, in Burnett's excellent experiments, which were devised to show the existence and nature of the co-conscious phenomena, and only incidentally used for a comparative study, the tests of comparative abilities differed in instructions, difficulty of material, and even in the amount of time, given in the two states.(**82**) (d) There have been too few subjects, often of a doubtful neurotic kind; too few sessions; too limited a field of investigation. The studies of Claparède (**83a**) and others suffer from these limitations. For example, Travis apparently allowed only one session in reverie and two in the normal state for the determination of auditory thresholds, and then correlated the results with suggestibility as judged by from three to five judges, who did not always judge unamimously. There are on record some objective experiments on suggestibility (using methods instituted by Binet,(**83b**) by Edwards, (**84**) McGeoch,(**85**) Sherman,(**86**) and best of all by Serog.(**87**) (e) The patent fact of wide variations in the successive performances of any given task by a normal subject, even in waking life, has never been taken into account in interpreting the results of published investigations.(**88**) It is a gratuitous assumption in the face of all the possible factors of chance bodily and mental states, *e.g.*, factors like mood, interest, tiredness, training under hypnosis, stage-fright, etc., to select one factor, that of being awake or being hypnotized, as the cause of the differences (regardless of the direction in which the differences run!) which have been found in the two series of tests.

However, in spite of the fact that so far no real differences in abilities (except in regard to endurance and long-past memory) have been substantiated, the hypnotic tradition of greatly augmented functions is being perpetuated by those who mention the subject. Thus Rivers,(**89**) Bleuler,(**90**) Morgan,(**91**) Travis,(**92**) and many others assume great differences in favor of hypnosis. McDougall, on the other hand, inclines seriously toward denying the popular conception.(**93**) Exceedingly strong evidence for radical changes in the functioning of the smooth musculature in hypnosis will be taken up in a later article.

Theories of Hypnotism

Quite clearly a theory of hypnotism will depend on what characteristics are considered essential and what are considered non-

essential in the state of hypnosis. Present theories tend to consider hypnosis an entity made up of all the phenomena which appear in it, regardless of how they arise. Some of the current theories are as follows:

1. Physiological. In spite of the general uselessness of physiological theories,(**94**) nearly all writers have formulated them, from Braid, who speaks of the "derangement of the cerebrospinal centers"(**95**) to McDougall, who talks of "the correlative of all available neurokyne along the channels of one disposition."(**96**) The physiological theories of Alrutz, Boirac, and Kindborg have already been referred to on page 504f.

2. Hypnosis as dissociation. Whether this is theory or description matters little. Most writers, *e.g.*, Prince, McDougall, and Brown, consider the split in the personality as vertical, so to speak; Sidis (**97**), Bramwell (**98**), Baudouin (**99**), consider it a horizontal cleavage, inasmuch as they consider the resulting state in some sense subconscious.

3. The psychoanalytic theories are: As expressed by Freud, "the unconscious fixation of the *libido* on the person of the hypnotizer (by means of the masochistic component of the sexual desire)" (**100**); as expressed by Jones, a going back either to primary narcissism or to the father ideal (**101**).

4. Hypnosis as based on a specific instinct. Trotter (**102**), Rivers (**103**), McDougall and Bleuler (**105**), attribute suggestibility to the gregarious instinct. The last two base it specifically on the instinct of submission (**104**).

5. Hypnosis as a primitive instinct. Sidis makes hypnosis and sleep the two differentiated conditions arising out of a primitive rest-state (**106**). So, too, does Pavlov base sleep and hypnosis on a primitive form of inhibition, the difference being that more of the brain action is inhibited in sleep than in hypnosis (**107a**). Friederichs' archaic faith-attitude is apparently analogous to an instinct (**107b**).

6. Hypnosis as a form of affectivity. Gregg (**108**) says it is an appeal to the emotions, not to the unconscious; and Prideaux (**109**) states that it is affectivity (sentiment or complex) that gives conviction in hypnosis.

7. Hypnosis as an autosuggestively induced state. Baudouin considers hypnosis an outcropping of mental immobility arising from relaxation and characterized by attention which is void of effort.

8. Other theories are that it is a state of overattention (Münsterberg) (**111**) of sleep, of predominance of the subconscious, etc.

Miscellaneous Matters

Dangers of Hypnotism. Nothing new has been ascertained in regard to the dangers of hypnotism, moral or otherwise, since Braid found out that hypnotism was in every way harmless for normal persons (112). Now, as then, however, there are demurrers (113). Perhaps Baudouin has said the wisest thing on record in this regard: "Can persons be constrained by hypnotism to the performance of a bad action? . . . Yes, if the subject imagines this to be possible. . . . Books that point out the dangers of hypnotism are far more dangerous than hypnotism itself" (114).

Uses of Hypnotism. The uses of hypnotism have been periodically forgotten and rediscovered. Any text will give more uses than any hypnotizer will ever employ. The medical uses of hypnosis have been detailed by many writers. McDougall (115) has pointed out its use as a method of experimental psychology, and Wells (116) has stressed its importance for instructional purposes in a course, say, in abnormal psychology.

Hypnotizability. Hypnotizability, in spite of various opinions, is as much an enigma as ever. Whether it is a correlate of extroversion, as McDougall says (117), or of real dissociation, as Brown avers (118), has not been proved. To state that above 90 per cent of the population can be hypnotized (119) does not clarify the situation; and besides demands such an attenuation in the state of hypnosis as to make it in some cases a mere relaxation or reverie, or an all-too-apparent coöperation. Morgan's and Travis' test for hypnotizability and dissociability have been referred to already (120). Braid and Bramwell found that hypnotizability and normality (121) were in direct ratio; whereas Brown finds the opposite (121); and Mosse finds hysterical symptoms in children accompanied by easy hypnotizability (122).

Conclusion

A survey shows that what phenomena are essential to hypnosis is yet to be determined. This is likely because of the doubt herein cast upon the spontaneous appearance of catalepsy, post-hypnotic amnesia, and *rapport.* Equally questionable is the tradition that physical and mental functions (aside from increased resistance to pain, greater endurance of fatigue, and increased ability to remember long-past events) are augmented. A strictly controlled comparative method is needed to establish the validity of the phenomena on which

the present theories of hypnotism insecurely rest. Although hypnosis is not an artifact of coöperation, in the sense that those who deny its existence claim, it is to a large extent an artifact of autosuggestion, at least as involved in expectation.

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